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Each Notice is illustrated by a portrait of the subject.

AJAllmand

ARTHUR JOHN ALLMAND

1885-1951

This biography of Arthur John Allmand is compiled from the personal record he left with the Society, from appreciations of pupils and friends, and from the recollections of the author who worked side by side with him in Liverpool and maintained a life-long friendship with him.

The plan of the biography is first to take the reader through the answers to the Society's questions followed by an appreciation of his work and character.

Allmand's favourite dictum on a professor of one of the natural sciences was that he might be a great researcher, a great administrator or a great teacher. Sometimes he might shine in two of these categories but to do so in all three was unattainable for most people. This should be borne in mind, as it was undoubtedly the pattern to which his life conformed.

Allmand's father, Frank Allmand, was a flour miller of Wrexham, a quiet pale man like his son, who attained the great age of ninety years. His mother's maiden name was Thomas and her father was a timber merchant of Wrexham.

The Allmands came from the Malpas district of Cheshire. The border between England and Wales is not all that firmly drawn. Allmand may fairly be claimed as a Welsh dominant; in character and outlook he was wholly so. One of his great grandmothers was a Frenchwoman.

He had a happy childhood in and near Wrexham, was naturally very studious and an omnivorous reader with a special taste for the Celtic tradition. Geoffrey of Monmouth, the Mabinogion, Malory and Welsh folk songs for example.

The family were not well off during his youth. Originally his father had a water mill, and later a steam mill; the shadows, however, were lengthening for the small millers. On top of this his father's mill was burnt down and he was reduced to becoming a corn and flour dealer. To better this he bred large white pigs very successfully from the show point of view, gaining championships at the 'Bath and West' and many other prizes. The financial return, however, was not very great. He also attempted to grow mushrooms in the basement of his burnt-out mill but without any great success. All this was confirmed by the impression he made as a man struggling hard against misfortune.

According to Allmand, his mother's family were more prosperous and talented than his father's, but the latter's courage and persistency were undoubtedly reflected in his son.

Allmand's holidays, in his youth, were all spent either in Welsh seaside resorts or in the depths of the country. He had some knowledge of the Welsh language, but did not read it with any facility. His accent and pronunciation were to the ear of an Englishman at any rate, racy, of the soil.

He went to a Dame's School in Wrexham from an early age, till 1894, and from that year till 1898, he was at Alleyn's School at Dulwich, living with an aunt in London. In the latter year, owing to his aunt leaving London, he returned to Wrexham where he went to the Grove Park school.

Scholarships played no part in his early career except one which waived his fees in Dulwich, which drew down on him the displeasure of the headmaster when he had to leave in 1898.

The headmaster of the Grove School wanted all his bright boys to go to Oxford.

Allmand, by this time, had determined to become a chemist under the following circumstances. His father had shown him an article in the *Scotsman* by Professor Tapp of Aberdeen, lauding the chemical industry as a career. The paper had been sent home by one of his aunts, the wife of another Aberdeen professor. This fired Allmand's imagination and made him determine on a chemical career. He also formed the opinion that he could learn chemistry better in one of the newer universities, and stuck to his guns despite the disapproval of his headmaster. Allmand was always capable of forming his own opinion and sticking to it.

He took his matriculation in 1901 (London), and was placed ninth in Honours, and in 1902 entered the University of Liverpool with three Scholarships, the Gilchrist, the Tate Scholarship in technical science and one from the Denbighshire Education Committee.

At the time of Allmand's entry, the University of Liverpool was in process of formation from the old Victoria University whose constituent colleges were at Liverpool, Manchester, and Leeds. The new University at that time had a remarkable and brilliant body of men on the staff: Sherrington, Ronald Ross, Sampson, Kuno Meyer, Pares, Elton, Augustus John, and Reid Dick, to name only a few. They were a most accessible lot. Apart from his chemistry, and particularly after his graduation, Allmand at once fell under the influence of Bernard Pares, from whom he learnt Russian, Kuno Meyer, who taught him some Celtic philology, Sampson, naturally, with his Welsh and gipsy affiliations, also. Oliver Elton was so much impressed by his literary ability that he asked him to join his staff.

Now returning to his chemical career. He took his intermediate examination in chemistry, physics and mathematics, not doing enough of the latter two, as he always admitted in later life.

The professor of chemistry at this time was James Campbell Brown, who, though not a particularly effective lecturer, was a man of considerable influence, and his integrity and general outlook commanded respect. Titherley, the lecturer in organic chemistry, was already famous for his discovery of sodamide and his lectures were brilliant and most attractive.

It says much for Campbell Brown that he was a devoted admirer of Willard Gibbs the great American.

In those days students were not expected necessarily to know much chemistry when they arrived at the University. In any case all had to take the first year lectures which were sometimes nearly a riot. During this period Allmand, Warrington Yorke, and Freeth, always sat side by side. Allmand was the quiet member of the trio. Allmand's undergraduate career was free from any ups and downs. With his intense devotion to work, his great ability and excellent memory he sailed through everything and obtained, in 1905, a first-class honours degree in chemistry.

It was the arrival of F. G. Donnan in 1904, in the new chair of physical chemistry, which influenced his career so profoundly. Donnan, an Ulsterman of the highest ability, charm and capacity for friendliness, fresh from working with van t'Hoff and Ostwald, immediately captured us all, and we became ardent physical chemists on the spot.

In these days, there were only six candidates for honours in chemistry, Allmand, Barker, Batey, Freeth, Hicks, and Purser. Our contacts with our professors were of a frequency and character which would be impossible nowadays on account of sheer weight of numbers.

Donnan's method of inducting young men into research, there was no Ph.D. degree, was to get them to repeat some paper recently published by one of the best known men of the day. He introduced us to all the new techniques till then quite unknown to us. Thermostats, capillary electrometers, the phase rule techniques and so forth. Besides all this, he gave us a broad European outlook which stayed with most of us for the rest of our lives.

Allmand was immediately attracted by electrochemistry. A glance at his bibliography, herewith, will show that it was his major scientific predilection though he had numerous and successful incursions into photochemistry and adsorption.

Returning to our chronology, after taking his Honours degree in 1905, Allmand began research under the aegis of Donnan. The latter, for the next few years, in addition to his ordinary duties, had to play a substantial part in the design and equipment of the Muspratt Laboratory of Physical Chemistry. In this he was ably and devotedly assisted by Allmand who thereby gained an experience which was always useful to him. His progress was marked by an M.Sc. degree in 1906, and a D.Sc. in 1910. In the latter year he was awarded an 1851 Exhibition which took him to Karlsruhe under Haber in 1910-11 and to Dresden under Luther in 1911-12.

Commenting on his experience in Germany, Allmand said that while liking both Haber and Luther well enough and having the greatest respect for their work, they had little personal influence on him. Neither of the researches in which he was engaged came off and as he quite over-modestly suggested, 'a better experimenter would have learned more than I did from the failures.' Parenthetically, surely this was due to his eyesight; as a child his eyes were normal, but as an aftermath of scarlet fever, he had to wear very thick glasses which were a great handicap to any manipulation or the playing of games.

In 1912 and 1913 Allmand became Donnan's research assistant in Liverpool and accompanied him to London when the latter followed Sir William Ramsay in the Chair of Chemistry in University College.

From 1913 to 1919 Allmand was assistant lecturer and demonstrator in the University of Liverpool.

In the summer of 1914 Allmand and Donnan, travelling in Germany, were only able to escape by the skin of their teeth by way of Poland, Russia and Sweden.

In October 1914 Allmand sent a telegram to Freeth, who was serving in the 5th (E. of C.) Battalion the 22nd (Cheshire) Regiment, T.A., 'Can you assist me to obtain a commission in the Cheshire Regiment'. To which the reply came, 'Yes, if you will shave off your beard'. He was commissioned in January, 1915, and took to soldiering in that quiet, conscientious, tolerant, efficient way in which he did everything else.

In the spring of 1915 he joined the 5th Battalion near Ypres; they subsequently went to the Somme.

After the beginning of chemical warfare every officer with any chemical knowledge was roped in to assist in overcoming it. Allmand was seconded from Regimental duty to 'Gas' Services R.E. as Assistant Chemical Adviser to the Third Army. He was awarded a Military Cross in 1916. In that year he was made Chemical Adviser to the Fourth Army, in 1918 he held the same appointment with the Second Army, was in the Army of Occupation and demobilized with the rank of Major in 1919.

In 1919 he became Professor of Chemistry in King's College, London, becoming Dean of the Faculty and senior professor till his retirement in 1950.

In 1920 Allmand married Mlle M. M. S. Malicorne at St. Mandé (Seine). His parents-in-law were Normans from the St. Lô district; small landowners, professional and service people well describes them. There were three children of the marriage, a daughter named Marguerite, who is married to a Civil Servant named John Murphy, a son Michael, who at the age of 20 was killed in the war, serving in a Ghurka Battalion with the Chindits. He was awarded a posthumous V.C.: we will return to him later. The younger son, Christopher, is still being educated.

About the time of his marriage he joined what one of the orators at his memorial service termed the 'Roman Obedience'. This to Allmand was simply 'the Catholic Church'. It was a landmark and turning point in his career. It did not surprise the writer at all. Allmand was naturally a religious man, and had found the Church and the creed which claimed and held his devotion for the rest of his life. The Pope made him a Knight of the Order of Saint Gregory in 1950.

Allmand was a Vice-President of the Chemical Society. He was most appropriately President of the Faraday Society, 1947-48. He was an Honorary Fellow of the Polish Chemical Society. He was elected into the Royal Society in 1929. He was a Fellow of the Royal Institute of Chemistry and belonged to the Electroplaters Technical Society and for many years was a member of the American Chemical Society and the Deutsche Bunsen Gesellschaft.

As already stated he retired from the Chair at King's College and only survived for one year, dying after an operation on 4 August 1951.

He altered very little in appearance throughout his life. He was a grave youth and much shyer than many people realized. Physically he was not really strong and kept going largely by his will power. The writer has a vivid recollection of a long walk with him in North Wales in the spring of 1903 or 1904. Intending to go about twenty-five miles, owing to a mistake in direction we found ourselves at tea-time with twenty or more miles to go. In all we walked fifty miles in the day. During the last ten miles he became exhausted and characteristically refused any help until about the last few miles. He just drove himself along.

All information received from his friends, pupils and associates points in the same direction: Allmand working himself again and again to the verge of breakdown in doing what he regarded as his duty to the college and above all to his students. He was a great teacher, his lectures were a model of clarity, and the pains he took were beyond belief. He never sought the limelight at all.

His researches were devised to educate his students, many of whom have attained great distinction in academic and technical life.

To quote from one of them in a great industrial research laboratory: 'As an administrator he was actively concerned with the most minute details particularly where they concerned the well-being of an individual. He was indeed meticulous in all he undertook. As a lecturer the subject matter was excellent, but he had a very quiet voice which did not carry well, and he talked at an extremely fast rate. As a teacher of research he was outstanding.

'He would come into each research laboratory once or at the most twice a week and ask each worker how things were going. You would tell him and he would often say, "what do you think is the mechanism"? As young men full of enthusiasm we propounded our theories, which often were obviously absurd if we could have realized it.

'He never told us that our ideas were impossible, but instead he gradually turned the conversation until in a quarter of an hour's time the student said his views were hopeless. He developed his students' reasoning powers and made them think and talk. Truly a very great attribute. He had a phenomenal memory and would quote author, journal, year, and page number of quite abstruse papers on a wide variety of subjects embraced by physical chemistry.

'He was indeed a glutton for work and toiled on till the early hours of the morning, so much so that he had several breakdowns or near breakdowns and was forced to take life more easily.'

The director of research in one of our great chemical companies says of him:

'Before 1914 Allmand's main interest had been electrochemistry. After the war he added to this a growing interest in photochemistry and by 1924 had several researches in both these branches of physical chemistry.

'Debye and Hückel had just made their great contributions to the theory of solutions of electrolytes, but, thereafter, for a time, the subject became somewhat arid, progress with the theory of concentrated solutions, which Allmand would have liked to advance, became very slow and work on the subject leaned heavily in the direction of thermodynamic treatment and development.

'Photochemistry, on the other hand, was still at an early stage, for its development had waited for the development of the quantum theory and for improved instruments and techniques. It afforded unlimited scope for careful and precise measurements and was constantly throwing up new problems. I remember that in those days much work was being done by Allmand on the hydrogen-chlorine reaction, then a most baffling problem.

'Photochemistry brought its connexions with spectroscopy, kinetic theory and reaction kinetics that added greatly to the breadth of Allmand's learning and the interest of his school.'

Allmand soon came to be regarded as a leading photochemist. He maintained his interest in electrolytes and usually had students working on this subject and kept up a remarkable knowledge of its literature by a prodigious amount of reading.

'He was extremely helpful and considerate to his students. He expected hard work from them, and in return he worked extremely hard both in physical chemistry, which was his absorbing interest, and in the interest of his students. He was especially conscientious about his supervisory duties and his research students were visited by him three or four times a week. He was a familiar figure, with his fiery hair and large round, gold-framed spectacles.

'Usually he was dressed in a blue suit, flannel shirt and regimental tie.

'He confined himself to the business in hand and though he would discuss a problem with great animation, bringing to it evidence of wide and deep knowledge, he did not linger, during the day, to discuss anything but work. He was too busy a man, through being in the best sense a slave to duty. He had an enthusiasm for army life and training, rather unusual at that time, which perhaps reflected the great influence his war-time service had had upon him.

'Allmand was excellent as a lecturer, clear, precise and elegant in method, and his clarity was used to take the fullest advantage of the careful and thorough preparation he so evidently and invariably gave to his material.

'Allmand had all the qualities needed by a university professor, to help him to fulfil, and more than fulfil, his duty to the University, the college and its members, and the many professional bodies he was connected with. A man of greater genius might have left a greater mark on the development of his subject, *but, only, I think, at the expense of the trouble which Allmand took over the teaching and training of men, for he did see that during those years at King's College physical chemistry was well taught and research was well supervised.*'

The above quotations amply prove the final conclusions of the writer, namely, that Allmand's main working life was dominated by his intense religious conviction that his duty was to serve his country, his university and his students to the greater glory of God.

This conclusion is supported by the statement of Sir Ernest Barker in his book *Age and Youth*, which was brought to the writer's notice just as this biography was completed.

Speaking of King's College, London, he said, 'in chemistry there was that rare spirit, saint as well as chemist whose memory is a benediction, A. J. Allmand'.

Finally, as has already been stated, the death in action of Allmand's elder son Michael, was a most grievous and terrible blow, alleviated by its gallantry. He was awarded a posthumous Victoria Cross with the following citation:

'Captain Allmand was commanding the leading platoon of the 6th Gurkha Rifles on 11th June, 1944, when the Battalion was ordered to attack the Pin Hmi Road Bridge.

'The enemy had already succeeded in holding up our advance at this point for twenty-four hours. The approach to the bridge was very narrow as the road was banked up and the low lying land on either side was swampy and densely covered in jungle. The Japanese, who were dug in along the banks of the road and in the jungle with machine guns and small arms, were putting up a most desperate resistance.

'As the platoon came within twenty yards of the bridge, the enemy opened heavy and accurate fire, inflicting severe casualties and forcing the men to seek cover. Captain Allmand, however, with the utmost gallantry, charged on by himself hurling grenades into the enemy positions and killing three Japanese himself with his kukri.

'Inspired by the splendid example of their platoon commander the surviving men followed him and captured their objective. Two days later Captain Allmand, owing to casualties among the officers, took over command of the company and dashing thirty yards ahead of it through long grass and marshy ground, swept by machine-gune fire, personally killed a number of enemy machine gunners and successfully led his men on to the ridge of high ground that they had been ordered to seize.

'Once again, on June 23rd, in the final attack on the railway bridge at Mogaung, Captain Allmand, suffering from trench foot, which made it difficult for him to walk, moved forward alone through deep mud and shell-holes and charged a machine-gun nest singlehanded, but he was mortally wounded and died shortly afterwards.

'The superb gallantry, outstanding leadership and protracted heroism of this very brave officer were a wonderful example to the whole Battalion and in the highest tradition of his regiment.'

On this stirring note we can leave our friend Allmand secure in the affection, admiration and respect of generations of students and of all who knew him.

F. A. Freeth.

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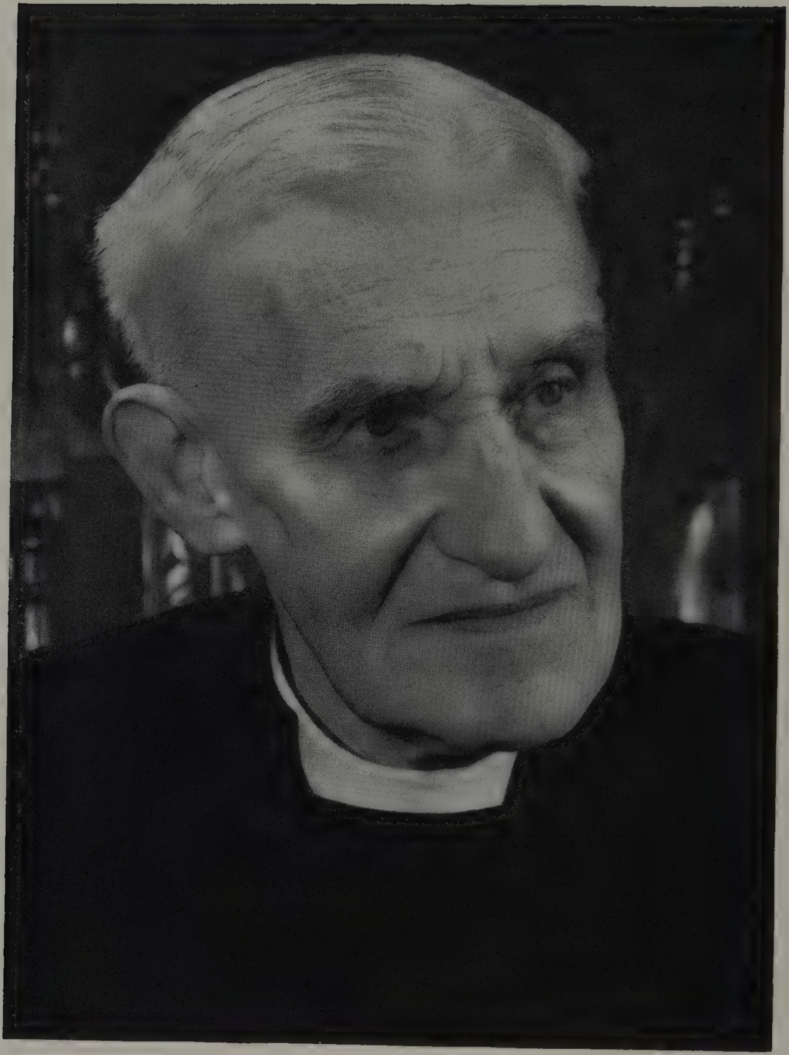

E. W. Birmingham

ERNEST WILLIAM BARNES

1874-1953

The father of Ernest William Barnes was John Starkie Barnes, a native of Accrington in Lancashire, whose forebears and relatives were all engaged in the cotton trade. Mr Barnes became an elementary teacher in the sixties of the last century, and at an early age was appointed a headmaster. His wife, Jane Elizabeth Kerry, who came of an agricultural family in the small Oxfordshire town of Charlbury, was at the time of their marriage headmistress of the associated school for girls. They had a family of four sons, of whom the eldest, the subject of this notice, was born at Birmingham on 1 April 1874: the second, Arthur Stanley (1875—) became M.D., D.Sc., F.R.C.P., and Dean of the Medical Faculty in the University of Birmingham; the third, Alfred Edward (1877-1916) won a classical scholarship at Trinity College, Cambridge, was called to the Bar, and became an official of the Local Government Board; the youngest, James Sidney (1881-1952), was also a scholar of Trinity, was Third Wrangler in the Mathematical Tripos, and entered the Admiralty: he rose to be Deputy Secretary and to be awarded the C.B. and the K.B.E.

Mr J. S. Barnes, after holding more than one headmastership, became Clerk to the King's Norton School Board, and, about 1883, an Inspector of Schools in Birmingham, a position that he occupied throughout the rest of his working life.

Ernest William Barnes was educated first at King Edward's School, Birmingham, where he came under the influence of the headmaster, A. R. Vardy, and the chief mathematics master, Rawdon Levett. In 1892 he won an entrance scholarship in mathematics to Trinity College, Cambridge; he was bracketed Second Wrangler in 1896, was President of the Union in 1897, was placed in the first division of the first class in Part II of the Mathematical Tripos in 1897, and was awarded the first Smith's Prize in 1898. In the latter year he was elected to a Trinity Fellowship, which in 1902 was combined with a college Lectureship and in 1908 with a Tutorship. He graduated Sc.D. of the University in 1907.

Barnes's earliest paper (1)* was a new proof of a theorem due to Picard regarding the integrals of linear differential equations with doubly-periodic coefficients.

In 1899 he wrote a long memoir (2) on the gamma-function, in which he gave the asymptotic expansion of this function at infinity for both real and complex values of the variable, and showed that the gamma-function and Bernoulli's functions can be represented as special cases of the same integral: he proved, moreover, that the gamma-function cannot be the solution of a differential equation whose coefficients are not functions of the same nature. This paper was followed in the same year by (3), in which he studied the G-function, a generalization of the gamma-function which satisfies the equation $G(z+1) = \Gamma(z)\ G(z)$; he showed that it can be expressed as a double product

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* The numbers in brackets refer to the list at the end of this notice.

$$G(z) = e^{r(z)}\ z \prod_{m,\,n\,=\,o}^{\infty}{}' \left\{\left(1+\frac{z}{m+n}\right) \exp\left[-\frac{z}{m+n}+\frac{z^2}{2(m+n)^2}\right]\right\}$$

where r is a quadratic function of z, and the accent denotes that the pair $m=0$, $n=0$, is to be excluded from the infinite product; he also gave integral expressions for it. He then (4) examined a generalization of the G-function, which had been discovered by Alexejewsky in 1894, and which he named the *double gamma-function*, since it bears to Weierstrass's sigma-function the same relation as $\Gamma(z)$ bears to sin πz. It satisfies the equation

$$G\left(\frac{z+1}{\tau}\right) = \Gamma\left(\frac{z}{\tau}\right)\ G\left(\frac{z}{\tau}\right)$$

where τ is a constant, and it can be represented by the doubly-infinite product

$$G\left(\frac{z}{\tau}\right) = \exp\left(a\frac{z}{\tau}+b\frac{z^2}{2\tau^2}\right)\frac{z}{\tau}.$$

$$\prod_{m,\,n\,=\,o}^{\infty}{}' \left[\left(1+\frac{z}{m\tau+n}\right) \exp\left\{-\frac{z}{m\tau+n}+\frac{z^2}{2(m\tau+n)^2}\right\}\right]$$

where a and b are functions of τ only. If a and b are suitably chosen, it satisfies the difference-equations

$$G\left(\frac{z+1}{\tau}\right) = \Gamma\left(\frac{z}{\tau}\right)\ G\left(\frac{z}{\tau}\right)$$

$$\text{and } G\left(\frac{z+\tau}{\tau}\right) = \left(2\pi\right)^{\frac{1}{2}(\tau-1)}\ \tau^{-z+\frac{1}{2}}\ \Gamma(z)\ G\left(\frac{z}{\tau}\right).$$

The product of four particular functions can be expressed in terms of a Jacobian theta-function. In a long memoir (5) he changed the notation slightly so as to have symmetrical difference-equations involving periods ω_1, ω_2, instead of 1, τ, and worked out many more properties of the double gamma function and related functions. By analogy with the usual definition of the simple Bernoulli function $S_n(a, \omega)$ (whose coefficients involve the Bernoullian numbers) as that polynomial of the $(n+1)$th degree in a which satisfies the difference-equation $f(a+\omega)-f(a) = a^n$ and vanishes with a, we can define the *double Bernoulli function* ${}_2S_n(a/\omega_1, \omega_2)$ to be that polynomial of the $(n+2)$th degree in a, which vanishes with a and satisfies the difference-equation

$$f(a+\omega_1)-f(a) = S_n(a, \omega_2)-S'_{n+1}(a, \omega_2)/(n+1).$$

This function is symmetric in the parameters ω_1, ω_2. Barnes proved that, apart from a linear and a constant term, ${}_2S_n(a/\omega_1, \omega_2)$ is the only polynomial that satisfies the difference-equation

$$f(a+\omega_1+\omega_2)-f(a+\omega_1)-f(a+\omega_2)+f(a) = a^n.$$

He proved many of its properties, and applied them to established theorems regarding the double gamma function, showing that it can be represented as a singly-infinite product of ordinary gamma-functions, multiplied by quadratic exponential functions of the variable: this form corresponds to the representation of elliptic sigma-functions by infinite products of circular functions. He then discussed its connexion with the Weierstrassian elliptic functions, and with a double Riemann zeta-function defined by

$$\zeta_2\left(s, a/\omega_1, \omega_2\right) = \frac{i\Gamma(1-s)}{2\pi}\int \frac{e^{-az}(-z)^{s-1}\,dz}{(1-e^{\omega_1 z})(1-e^{\omega_2 z})}$$

where the integration is taken along a curve beginning and ending at infinity and enclosing the origin. This function is the simplest solution of the difference-equation

$$f(a+\omega_1+\omega_2)-f(a+\omega_1)-f(a+\omega_2)+f(a) = a^{-s}.$$

Stieltjes had proved that the asymptotic expansion for $\Gamma(x)$ is valid for all values of $|\arg x|$ less than π. Now Barnes, using an idea suggested by Mellin, extended the result to the multiple gamma-functions. The theory of multiple Bernoulli functions, multiple Riemann zeta-functions, and multiple gamma-functions, was studied further in (12).

In (9) he showed that in terms of the double gamma-function it is possible to express the coefficients of capacity of two non-intersecting electrified spheres.

Barnes's next considerable work, (6), was a study of integral functions, a term which he used as a translation of the French *fonctions entières*, i.e. holomorphic transcendental functions: and it was carried further in (8) and (13). He investigated the asymptotic expansions of the logarithms of integral functions of finite order defined by Weierstrassian products, and suggested that such investigations, which characterize the essential singularity at infinity, might lead to a classification of integral functions, relating the classification to Laguerre's introduction of *genre* and Borel's notion of *order*. Barnes's expansions were obtained for functions of simple and multiple linear sequence, these terms being defined as follows: a *simple* integral function is one that may be expressed as a Weierstrassian product whose nth zero a_n depends solely upon n and definite constants, and which is such that the law of dependence of a_n upon n is the same for all but a finite number of zeros; functions of *multiple linear* sequence are functions whose general zero is of the type $f(\alpha+n_1\,\omega_1+\ldots+n_r\,\omega_r)$, α and the ω's being constants and the n's being the whole numbers that define the particular zero.

The investigation was based on the theory of divergent series: in (6) Barnes developed for this purpose a theory of Borel, who had shown that a power-series with a finite radius of convergence can by means of a certain integral be interpreted for values of the variable outside the circle of convergence. The fundamental procedure consisted in applying the asymptotic expansions of the Euler-Maclaurin sum-formula to a transformation by logarithmic expansion of the function investigated. The terms of the double series which arose in this way were rearranged, and were then summed by an application of Fourier's theorem. Throughout the investigation no attempt was made to determine remainders for the asymptotic expansions. In (8) he further studied the properties of Taylor's expansions of integral functions, and in (13) he examined particularly the asymptotic expansions of functions of multiple linear sequence. As we shall see, the investigations on integral functions defined by Weierstrassian products were continued in (17) and (18).

In a short note (7) published about this time he proved that when $-\pi < \theta < n$, the sum of the Fourier series

$$\sum_{s=-\infty}^{\infty}{}' \frac{(-1)^s e^{si\theta}}{s^{n+1}}$$

can be expressed in terms of the nth Bernoullian function.

In (11) he gave an improved proof of the formula for Euler's constant,

$$\gamma = \int_0^1 \frac{dz}{z}\,(1-e^{-z}-e^{-1/z}).$$

In 1903 and 1905 he discussed the Euler-Maclaurin sum-formula

$$\sum \phi(x) = C + \int^x \phi(x)dx - \tfrac{1}{2}\,\phi(x) + \frac{B_1}{2_1}\,\phi'(x) + \ldots\,;$$

in (10) he generalized the analysis connected with it and considered it from the point of view of the theory of asymptotic series: and in (17) he gave a fresh demonstration of the conditions under which his extensions of the formula were valid, and obtained a new form for the remainder.

In (14) he entered a new field, investigating the functional nature of the solution of the homogeneous linear difference-equation of the second order with linear coefficients,

$$(a_2x+b_2)\,f(x+2)+(a_1x+b_1)\,f(x+1)+(a_0x+b_0)\,f(x) = 0.$$

It was shown that in the general cases the solution leads to the complete system of hypergeometric functions studied by Kummer and Riemann; it may be written

$$f(x) = \tilde{\omega}_1(x) f_1(x) + \tilde{\omega}_2(x) f_2(x)$$

where $\tilde{\omega}_1(x)$ and $\tilde{\omega}_2(x)$ are arbitrary simple periodic functions of x of period 1, while $f_1(x)$ and $f_2(x)$ are one-valued meromorphic functions of x with sequences of poles of similar type. As applications he considered the linear difference-equations for the Legendre and Bessel functions.

In (15) and (16) he studied linear difference-equations of the first order (particular cases of which had occurred in his work on ordinary and double gamma functions)

$$P(x+1) - P(x) = \chi(x).$$

The most simple solution of a linear difference-equation of the first order whose coefficients are meromorphic functions is, in general, a one-valued function with sets of simple sequences of poles tending to infinity. Barnes showed, in connexion with the above difference-equation when $\chi(x)$ is a one-valued analytic function, that, in general, its solution cannot be a solution of any differential equation of finite order unless either: (a) the coefficients of the latter are obtained by differentiation from the solution itself, or (b) we can, from these coefficients and the function $\chi(x)$ and its derivatives, by the fundamental process of forming finite differences coupled with a finite number of elementary algebraic operations, derive the solution itself. The cases of exception to this general theorem were considered. The theorem includes as a special case one proved by Hölder for the gamma function and extended by Barnes in (3) and (5) to the G and double gamma functions. It is important as showing that the linear difference-equation of the first order gives rise to new classes of transcendents which cannot be generated, as are so many functions, by differential equations.

In (16) he investigated in general the nature of the functions defined by linear difference-equations of the first order, considering specially: (i) the existence of a solution, (ii) its analytical expression, (iii) its place among transcendental functions. Let the equation be written

$$\phi(z) f(z+w) - \chi(z) f(z) = \psi(z)$$

where $\phi(z)$, $\chi(z)$, and $\psi(z)$ are assumed to be analytic functions of z.

This may at once be reduced to two others of simple type:

$$\frac{f_1(z+w)}{f_1(z)} = \frac{\phi(z)}{\chi(z)} \tag{A}$$

and

$$f_2(z+w) - f_2(z) = \frac{\psi(z)}{\chi(z)} f_1(z) \tag{B}$$

with

$$f(z) = \frac{f_2(z)}{f_1(z)}.$$

He then regards (A) and (B) as the two fundamental equations, taking $\phi(z)$, $\chi(z)$ and $f(z)$ to be integral functions as studied in (6), (8), and (13).

He considers the solutions of these equations under very general conditions, and finally deduces the nature of the solution of the original equation.

In (18) Barnes returned to the asymptotic expansion of integral functions defined by Weierstrassian products, which he had considered in (6), (8), and (13), but he now approached the subject from a different point of view, avoiding dependence on the theory of divergent series and returning to the methods used in (2) and (5) to obtain the asymptotic expansions of the simple and double gamma functions. The procedure is an application of Pauchy's theory of residues, suggested by an investigation in which Mellin had applied it to the case of the simple gamma-function.

In (19) he continued the study of asymptotic expansions, considering now the asymptotic expansions of functions that are defined by Taylor series instead of Weierstrassian products. The fundamental procedure is based upon the following theorem: suppose that we wish to find an asymptotic expansion when $|x|$ is large for the integral

$$I = \frac{i}{2\pi}\int_{C} e^{-xz} f(z)\ (-z)^{\beta-1}\, dz$$

taken round a contour C (as used in the theory of gamma-functions) which encloses the origin and embraces an axis drawn from the origin to infinity along which $R(xz)$ is positive: $f(z)$ being a function which for values of $|z|$ less than l admits the convergent expansion

$$f(z) = \sum_{n=0}^{\infty} c_n(-z)^n.$$

Then the integral I admits the asymptotic expansion

$$\sum_{m=0}^{\infty} c_m \frac{i}{2\pi}\int_{C} e^{-xz}(-z)^{\beta+n-1}\, dz = \sum_{m=0}^{\infty} \frac{c_n}{\Gamma(1-\beta-n)\ x^{\beta+n}}.$$

Barnes considers various standard types of integral functions defined by Taylor series, and for each function finds the behaviour at infinity, applying his new methods of contour integration so as to get complete asymptotic expansions.

(20), (21), (22), and (23) are developments of (19). In (21) he studied specially the case $\beta = 1$ of the function

$$G_{\beta}(x, \theta) = \sum_{n=0}^{\infty} \frac{x^n}{\Gamma(n+1)\ (n+\theta)^{\beta}}.$$

In (22) he applied the investigation given in (19) specially to the functions defined when $|x| < 1$ by the Taylor series

$$\sum_{n=0}^{\infty} \frac{x^n}{(n+\theta)^{\beta}}$$

This function has a single singularity in the finite part of the plane, at $x = 1$: it is not an essential singularity, but the function is many-valued in its neighbourhood. He found its asymptotic expansion, and in the second part of the paper obtained analogous theorems for a function which is defined when $|x| < 1$ by an expansion

$$\sum_{n=0}^{\infty} \frac{x^n \chi(n+\theta)}{(n+\theta)^\beta}$$

where outside a certain circle, $\chi(x)$ admits an absolutely convergent expansion as a series of negative powers of x.

In (24) he obtained the asymptotic expansion of integral functions defined by the generalized hypergeometric series

$$F(x) = 1 + \frac{\alpha_1 \alpha_2 \ldots \alpha_p}{1.\, \rho_1 \rho_2 \ldots \rho_q} x + \frac{\alpha_1(\alpha_2+1)\ldots\alpha_p(\alpha_p+1)}{1.\,2.\, \rho_1\ (\rho_1+1)\ldots\rho_q(\rho_q+1)} x^2 + \ldots \qquad (q \geqslant p)$$

and this gave a simple proof of some theorems found earlier by Orr.

In all Barnes's papers from (25) onwards he uses integrals in which the integrand involves gamma functions of the variable of integration, with a path of integration which is parallel to the imaginary axis, with loops if necessary. Such integrals had been introduced originally by Mellin, but it was from the writings of Barnes that British mathematicians first realized how powerful a tool they were.

In (25) he gave a proof of Abel's theorem on the binomial expansion for a complex variable and complex index, by making use of the integral

$$\frac{1}{2\pi i} \int_L \Gamma(\phi - s)\ \Gamma(-\phi)\ x^\phi\ d\phi \equiv F(x, s)$$

where s and x are complex numbers, and x^ϕ is $\exp(\phi \log x)$: L is a path of integration which is parallel to the imaginary axis and cuts the real axis between $\phi = 0$ and $\phi = -1$ (with a loop, if necessary) so that s lies on the left of L. Whatever the value of $|x|$ may be, the integral is convergent for all values of s, if $|\arg x| < \pi$, and, moreover, is convergent for $|\arg x| = \pi$ when $R(s) > 0$. For $s = 0, 1, 2, \ldots$, the integral does not exist.

Barnes shows, that for all values of x and s, for which $|\arg x| < \pi$ and $s \neq 0, 1, 2, \ldots$, we have $F(x, s) = -\Gamma(-s)\ (1+x)^s$, where $(1+x)^s = \exp\{s \log(1+x)\}$ and $|\arg(1+x)| < \pi$, and that

$$(1+x)^s = 1 + \frac{s}{1} x + \frac{s(s-1)}{1.2} x^2 + \ldots$$

whenever the series is convergent.

In (26) he makes use of factorial series of the form

$$\sum_{n=0}^{\infty} \frac{\Gamma(n-s)}{\Gamma(n+1)\ (n+s)^{\beta}}$$

in an asymptotic expansion.

In (27) he gave a new development of the theory of the hypergeometric function, which avoided certain difficulties that are found in the works on this subject by Riemann and Pochhammer. He considers the contour integral

$$2\pi i \int_{-\infty i}^{\infty i} \frac{\Gamma(a+s)\ \Gamma(b+s)\ \Gamma(-s)}{\Gamma(c+s)} (-z)^s \, ds$$

where the path of integration is curved (if necessary) so as to ensure that the poles of $\Gamma(a+s)\ \Gamma(b+s)$, namely, $s = -a-n, -b-n, (n = 0, 1, 2, \ldots)$ lie on the left of the path, and the poles of $\Gamma(-s)$, namely, $s = 0, 1, 2, \ldots,$ lie on the right of the path. He shows that this integral represents an analytic function so long as $|\arg z| < \pi$, and by the theory of residues it is found that when $|z| < 1$, this function may be represented by the series

$$\sum_{n=0}^{\infty} \frac{\Gamma(a+n)\ \Gamma(b+n)}{\Gamma(c+n)\ n!} z^n.$$

When $|z| > 1$, the integral can be transformed into the sum of two power-series in z^{-1}, and thus Barnes obtained the result

$$\frac{\Gamma(a)\Gamma(b)}{\Gamma(c)} F(a, b : c : z) = \frac{\Gamma(a)\ \Gamma(a-b)}{\Gamma(a-c)} (-z)^{-a} F(a, 1-c+a; 1-b+a; z^{-1})$$
$$+ \frac{\Gamma(b)\ \Gamma(b-a)}{\Gamma(b-c)} (-z)^{-b} F(b, 1-c+b; 1-a+b; z^{-1})$$

when $F(a, b : c : z)$ denotes the hypergeometric series, and $|\arg(-z)| < \pi$.

In the course of the work the value of the integral

$$\int_{-\infty i}^{\infty i} \Gamma(\alpha_1+s)\ \Gamma(\alpha_2+s)\ \Gamma(\beta_1-s)\ \Gamma(\beta_2-s)\ ds$$

is found in terms of gamma functions; a result often called 'Barnes' lemma'.

In (28) he investigated the Associated Legendre functions $P_n^m(z)$ and $Q_n^m(z)$ with arbitrary indices n, m, i.e. the solutions of the differential equation

$$(1-x^2)\frac{d^2y}{dx^2} - 2x\frac{dy}{dx} + \left\{n(n+1) - \frac{m^2}{1-x^2}\right\} y = 0$$

for arbitrary values of n and m, proceeding in much the same way as in the definition of derivatives of arbitrary index. It is first shown, that the integral

$$I_m(x) = -\frac{1}{2\pi i}\left(\frac{x+1}{x-1}\right)^{\frac{1}{2}m} \int \frac{\Gamma(s-n)\,\Gamma(n+1+s)\,\Gamma(-s)}{\Gamma(1-m+s)}\{\tfrac{1}{2}(x-1)\}^s\,ds$$

satisfies the above equation, and likewise $I_{-m}(x)$. In this the path of integration is parallel to the imaginary axis, with loops when necessary in order to secure that all positive sequences of poles lie on the right of the path of integration, and all negative on the left. By means of this integral the functions P_n^m and Q_n^m with arbitrary complex indices n, m, are defined for all values of x outside a cut drawn from $x = -\infty$ to $x = +1$, and indeed we have

$$P_n^m(x) = -\frac{\sin(n\pi)}{\pi} I_m(x), \quad Q_n^m(x) = \tfrac{1}{2}\{I_m(-x) - e^{\pm ni\pi} I_m(x)\}.$$

A great many formulae, particularly various representations of P_n^m and Q_n^m by means of hypergeometric series, are obtained by taking the integral I_m along a closed path of integration and applying Cauchy's theorem. In the second chapter the asymptotic values of the functions are obtained for $n \to \infty$ and $m \to \infty$, and for various values of x. In the third chapter a generalization of Rodrigues's formula is given, and various recurrence-formulae obtained and definite integrals calculated.

The last work of Barnes's career as a professional mathematician, (29), is a short paper on generalized hypergeometric series. Let

$$F(\alpha_1, \alpha_2, \alpha_3 : \beta_1, \beta_2, 1) = 1 + \frac{\alpha_1\,\alpha_2\,\alpha_3}{\beta_1\,\beta_2\,1} + \frac{\alpha_1(\alpha_1+1)\alpha_2(\alpha_2+1)\alpha_3(\alpha_3+1)}{\beta_1(\beta_1+1)\;\beta_2(\beta_2+1)\;1.2} + \dots$$

the series being convergent when $R(\beta_1+\beta_2-\alpha_1-\alpha_2-\alpha_3)>0$, and the function being defined to be the analytic continuation of the series when the inequality is not satisfied. Then it had been shown many years before by Thomae, but was now shown much more briefly and elegantly by Barnes, that

$$F(\alpha_1, \alpha_2, \alpha_3 : \beta_1, \beta_2, 1)$$

$$= \frac{\Gamma(\beta_2)\;\Gamma(\beta_1+\beta_2-\alpha_1-\alpha_2-\alpha_3)}{\Gamma(\beta_2-\alpha_1)\;\Gamma(\beta_1+\beta_2-\alpha_2-\alpha_3)} F(\alpha_1, \beta_1-\alpha_2, \beta_1-\alpha_3 : \beta_1, \beta_1+\beta_2-\alpha_2-\alpha_3, 1).$$

The utility of the result depends on the fact that one of the series may be rapidly convergent while the other converges very slowly.

Barnes was elected a Fellow of the Royal Society in 1909.

His father, Mr J. S. Barnes, was a Baptist, active in Church work: but Ernest, when a boy of about fifteen at King Edward's school, joined the Anglican Church. In 1902 he was ordained, and from 1906 to 1908 was Junior Dean of Trinity. In 1915 he left Cambridge, when, on the nomination of Mr Asquith's government, he was appointed Master of the Temple. In the following year he married Adelaide Caroline Theresa, only daughter of Sir Adolphus W. Ward, Litt.D., F.B.A., Master of Peterhouse. In 1918 he was nominated by the Prime Minister, Mr Lloyd George, to a Canonry of Westminster, and in 1924, by Mr Ramsay MacDonald, to the Bishopric of Birmingham. He was a Select Preacher at both Cambridge and Oxford on many occasions.

His appointment to high office in the Church brought to an end his life as a professional mathematician. In 1933, however, he published a volume of Gifford lectures delivered in the University of Aberdeen, under the title *Scientific theory and religion: the world described by science and its spiritual interpretation.* The science which is expounded in this book is not, however, that branch in which he was an acknowledged master, namely pure mathematics, but a number of different branches, some of which he must have learnt late in life: for two of the most important of them had been discovered only about the time when he exchanged Trinity for London, and much of the information given is of a date more recent than his elevation to the episcopal bench. One cannot but marvel at the ability and industry which had enabled him, in the intervals of his duties as a Bishop, to become acquainted with such an immense field of new knowledge.

He received the honorary degree of D.D. from the Universities of Aberdeen and Edinburgh, and the honorary degree of LL.D. from the University of Glasgow.

In 1952 he retired, on account of ill-health, and on 29 November 1953 he died. He was survived by Mrs Barnes and two sons.

My grateful thanks are due to Mrs Barnes, to Dr Stanley Barnes, and to Professor W. N. Bailey, for much help in the preparation of this Notice.

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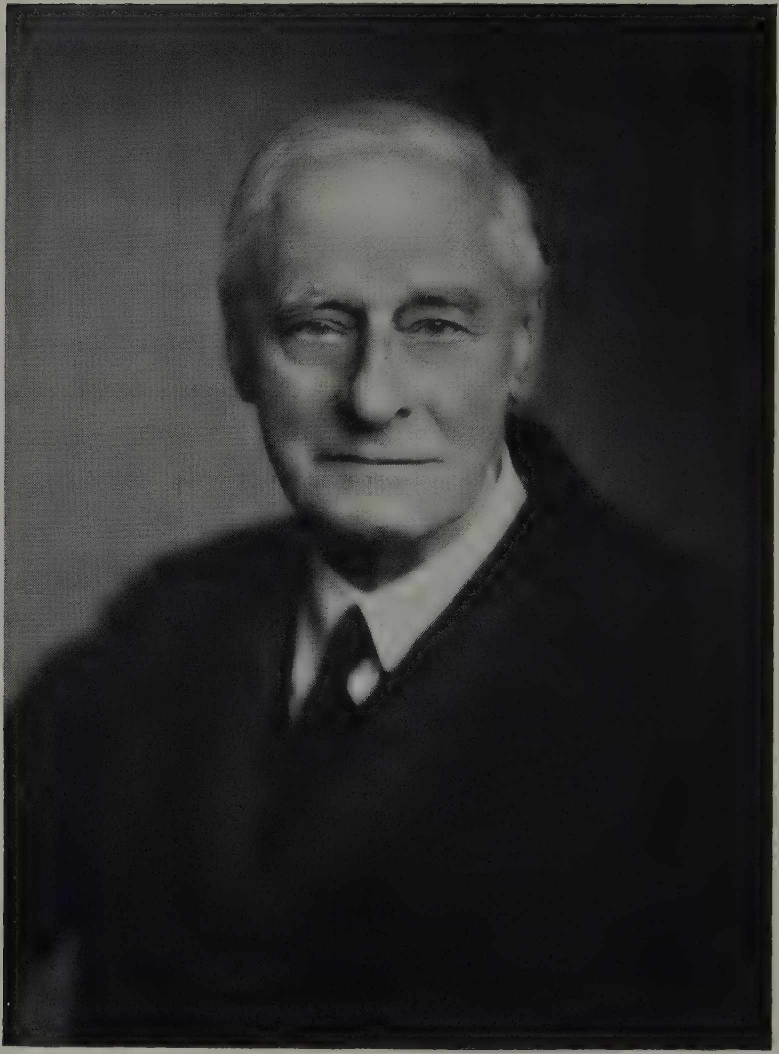

R H Burne

RICHARD HIGGINS BURNE

1868-1953

Richard Burne was elected to the Royal Society in 1927 because of his eminence as a comparative anatomist and biologist; he died in a nursing home, at Godstone, Surrey, on the morning of 9 October 1953, being in his 86th year. He was born at 122 Gloucester Terrace, London, W.2, on 5 April 1868. His father, Richard Higgins Burne, was a successful solicitor at No. 1 Lincoln's Inn Fields, London, W.C.2; his mother, Mayaretta Louisa Burne, was a distant cousin of his father. With the death of his elder brother Tom, in 1886, at the age of twenty, our Richard became an only child. All members of his ancestry were of pure English stock, being prosperous members of the professional or land-owning class. His father's people came from Staffordshire (Loynton Hall, near Newport), while his mother's people, for three generations, had been members of the medical profession in London. None of his forbears could claim a place in science; a niece of his father, Charlotte Sophia Burne, became the first woman President of the Folk-Lore Society. Richard's maternal grandmother was a daughter of Dr Henry Ford, Professor of Arabic at Oxford and Principal of Magdalen Hall. Dr Ford's wife was a niece of Dr John Butler, Bishop of Oxford, later of Hereford.

On 10 January 1893, when 24 years of age, Richard Burne married his first cousin, Louisa Joan Burne, the only child of his mother's brother, the Rev. John Butler Burne, vicar of Aldermaston and later rector of Wasing, Berkshire. The marriage was celebrated at St Nicholas Church, Wasing. There were three children of this marriage—Margaret Elizabeth, Thomas Oldbury and John Richard. His daughter Margaret, in 1922 wrote a novel, published by John Murray, in which she gave a description of the pleasant social life in England before the First World War, and at the same time drew a very sincere and vivid portrait of her father. Of his two sons, the elder, Thomas, was killed in the closing phase of the First World War, a loss which was keenly felt; the younger, John Richard, passed into the Royal Artillery but subsequently transferred to the Royal Army Pay Corps. When he died Richard Burne had five grand-children; a great-grandson had also arrived.

Burne had a happy childhood, much of it spent in a country home established by his father in Kent. In his ninth year he went to an excellent Dame school at Frant, near Tunbridge Wells, kept by a Mrs Adams and her two daughters. The scholars had the run of Eridge Park which served them as a nature reserve; they were encouraged to observe flowers and make drawings of them. From the Dame's school he went to a preparatory school at Twyford, near Winchester, kept by the Rev. Latham Wickham. The education there was of the regulation classical type. Mr Wickham confined his scholars to the playground except for a formal walk on Sundays; nature lessons were unknown. Nevertheless, Burne tells us in his narrative, 'I managed to start a collection of *Lepidoptera*. I can remember catching stag-beetles and keeping them from escaping by making them bite into a piece of soap.'

On leaving Twyford he went on to Winchester School, to E. J. Turner's house where his elder brother then was. There was a Natural History Society which for some reason did not appeal to him; but all the same he went on with his collection of insects. It was at this time he became interested in microscopic work, especially as an aid to his study of insects. The book which attracted him to microscopy and became the basis of his histological enquiries was Hogg's book on the microscope.* Also Mr W. B. Crofts, one of the masters at Winchester, gave him advice and practical help in the management of his microscope. At Winchester, however, in his time, there was no regular teaching in science, until the end of his years there (1885) when a man of very forceful personality, Mr Le L. Garbutt, came as science master and gave a course in chemistry. This course, which included practical work, appealed to Burne with a result which greatly surprised him—he was given a prize at the end of the course.

In 1886, at the age of eighteen, Burne entered Oriel College, Oxford, as a commoner. It was then he lost his elder brother Tom, who had preceded him both at Winchester and Oxford. After passing Moderations, it was decided that he should try for an honours degree in some scientific subject, and as chemistry was the only subject he had had any training in, he chose that. At the end of two years, under the tutorship of Mr Fisher, he took a third class in honours and graduated B.A. in 1889. Some years later he proceeded to the M.A. degree. He had no skill nor aptitude for ball-games, but during his time at Oxford he was in the Oriel VIII.

Burne was a man of extreme modesty and diffidence in his own ability, waiting for guidance in life to the suggestions which came from without rather than to any inspiration which might arise from his experience. Hence on his return from Oxford in 1889 there was a family debate (his father having died in 1882, at the age of 57) at his home in London as to what his next step should be, his source of income being assured. It so happened at this juncture that his uncle, at some public dinner, had as his neighbour Sir William Flower (1831-1899), then Director of the Natural History Museum, who, on being questioned, suggested that young Burne, having a taste for natural history, should take a course in practical zoology and so qualify as a candidate for a post in the National Museum. This advice was accepted, and, with this object in view, Burne went to the Royal College of Science and took the course in zoology under Professor George B. Howes (1853-1905) who, in 1885, had succeeded to Huxley's post. At this time Howes had an enterprising and able demonstrator—Martin F. Woodward; Burne worked under the personal guidance of Howes and Woodward and at the end of the course, duly passed the regulation examination.

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* *The microscope*, 1854. Jabez Hogg (1819-1899), M.R.C.S. (1850), Surgeon to Westminster Ophthalmic Hospital.

Professor Howes was keen on research; besides carrying on his teaching and his own morphological enquiries, he gave every encouragement to his pupils to follow in his footsteps. He perceived that Burne had uncommon delicacy of fingers and that his modesty hid a real and uncommon ability. Hence he invited his new pupil to stay on in his laboratory and put him on to several minor pieces of research.* Professor Howes was also building up a museum at this time; Burne helped in making preparations for it. Burne was also a skilled draughtsman.

At this time Professor Charles Stewart (1840-1907) was Conservator of the Museum of the Royal College of Surgeons of England and had just begun a very heavy task, the preparation of a new catalogue of the great collection wherein Hunter had sought to demonstrate the manner in which life-processes are carried out in all living things. There were over 3700 beautiful dissections by Hunter and his assistants preserved in spirit, illustrating, for example, how circulation is carried on in various orders; and so for many other systems such as respiration and locomotion. Never was such a rich series brought together as that in which Hunter sought to illustrate the diverse methods of propagation and organs of reproduction. The testes of the London sparrow and of the mole were shown at all stages of the sexual season. 'Adaptation' was the favourite subject with Hunter; in his *Physiological Series*, he had illustrated the manner in which the animal body was modified for creeping, walking, leaping, swimming, flying, etc. It was on this *Physiological Series* that Richard Owen spent twenty of the busiest years of his life in tracing the histories, identifying and giving descriptions of specimens left uncatalogued by Hunter. In 1892 Professor Stewart had undertaken to prepare a new catalogue and had mentioned to his friend Professor Howes his need of an assistant. Burne was warmly recommended, and so it came about that, in the summer of 1892, Richard Burne was given the minor post of anatomical assistant in the Museum of the Royal College of Surgeons of England. The Physiological Collection was then housed in two large rooms, known as Rooms IV and V. Little did Burne think, as he viewed the contents of these rooms in 1892, that his life's work lay in front of him.

It was during the time he began his work under Professor Stewart that I made the acquaintance of Richard Burne. In those days Professor and Mrs Howes kept open house on Sunday afternoons and evenings for special pupils in particular and for young adventurers like myself who found London cold and lonely. Those happy meetings at the home of the Howes near Turnham Green brought J. P. Hill, Burne, myself and many others together. At that time Burne was very fully occupied at the Museum of the Royal College of Surgeons. He found the scheme drawn up for a new catalogue of the *Physiological Series* was ambitious and far reaching. The descriptions of the specimens by Sir Richard Owen, which formed the first edition of the catalogue had to be checked and, where necessary, revised. Many blanks in the series, especially that which illustrated the development of the nervous system of invertebrates had to be made good by the preparation of new dissections. The preparation of such specimens fell to Burne's share of the work. Professor Stewart was a man of strong artistic tastes; he valued a specimen more for its artistic than from its illustrative qualities; he had high manual skill. The older and the younger men were alike in their outlook, ability and tastes. It was a most successful partnership.

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* At the end of this article is a list of Mr Burne's publications. Those numbered (1) and (2) are accounts of the pieces of research he did at this time. The numbers which occur in my text refer to the list of publications.

At first his contributions being of a malacological nature went to the Malacological Society, but with the coming of the 20th century his flow of papers began to go to the Zoological Society, most of his communications arising out of dissections made for the Royal College of Surgeons Museum. He threw himself more and more into the work and welfare of the Zoological Society as time went on. He was elected to its fellowship in 1890, served on its Council for six terms, held the office of Vice-President on two occasions; for sixty-three years he was a Fellow of the Zoological Society.

In no sense was Burne a specialist; he was equipped to investigate all macroscopic animal forms; his friend Martin Woodward was a keen student of the Mollusca and through him he did make some special studies of the lamellibranchs. For this reason he was asked to write the report on the lamellibranchs collected during the *Terra Nova* expedition (27). Most of his papers were occasioned by the particular section of the museum on which he was engaged such as those dealing with the olfactory organ of fishes (21, 24), the anatomy of the membranous labyrinth in sharks, a subject in which John Hunter had been interested, the connexion between the swim-bladder and ear in fishes, and the comparative anatomy of the internal ear of vertebrates (28, 29, 31). He made his observations on the membranous labyrinth the subject of three advanced lectures given under the aegis of the University of London in 1912 (29). In his earlier years he was handicapped in lecturing by a stammer; in his later years this almost disappeared. Three of his more important papers appeared in the *Philosophical Transactions* (37, 41, 42). In the third of these, he demonstrated the presence in the cod of an undescribed system of 'fine' vessels, serving a function which is still obscure. In the two others he deals with peculiarities in the blood vascular system of the porbeagle shark and with the ductless glands and lymphatic system of the angler fish. He collaborated with Professor J. P. Hill in an important paper on the foetal membranes and placentation of *Chiromys madagascariensis* (36). He also made many communications to the Anatomical Society of Great Britain and Ireland, of which Society he became a member in 1896. A comparatively recent communication is that which he gave to the Zoological Society in 1939, on the stomach, intestine, liver and pancreas of the okapi (43).

Late in 1907 Professor Stewart died at the age of sixty-seven and the office of Conservator of the Museum of the Royal College of Surgeons of England thus became vacant. Richard Burne had a definite claim to the office and he and I were among the candidates for the post. The Council of the College ultimately decided in my favour, but this never interrupted the friendship which Burne had so constantly shown me; we worked together in complete harmony but I admit that was more due to his native merit than to any natural quality in me. He accepted the post of Assistant Conservator and held it until 1912. He then begged to be relieved of his routine duties and so be in a position to devote all his time and energy to the Physiological Collection. The post of Physiological Curator was created for him and this he held until 1934, when at the age of sixty-six he asked the Council to relieve him of his curatorship. I am revealing something of a secret when I mention that on becoming Physiological Curator he offered to forgo his salary if Council would add it to the Conservator's emoluments. This generous thought was rightly turned down; Mr Burne received a 'honorarium' in place of his former salary. The Council of the College recognized the great service Burne had rendered to its Museum, and in 1925 made him the recipient of its Gold Medal—a rare award. When he retired in 1934 the Council gave him its thanks for the great services he had rendered to its Museum and asked him to accept a gift to be spent on a piece of plate or in what way he might choose. He chose to spend it on a cruise for Mrs Burne and himself to Greece. Like so many English people who have inherited wealth he was generous in all forms of gifts save those spent on self; as to these he may be said to have been mean.

On his retirement from the College Burne gave his voluntary services to the Institution he had prepared himself for at the out-set of life—the Natural History Museum, South Kensington. His connexion with the Natural History Museum was commemorated in 1952 by the publication of a *Handbook of Cetacean Dissections* (44) carried out in the years of his retirement.

Also characteristic of him were the form his public services took in the 1914-18 war. For a time he acted as special constable and then for some seven months served as a driver of an ambulance attached to the French Army on the Belgian front. He came home to join the Inns of Court Volunteers and worked three days a week as an unskilled labourer in Messrs Vickers factory at Erith. As has been already noted he lost his elder son in the closing phase of the war in the spring of 1918. He found his chief recreation in driving along country lanes in his family car.

Mrs Burne having died in 1945 and his two children married with houses of their own, Richard Burne, as he reached the 'eighties' made a home at Limpsfield, Surrey, to be near the home of his daughter, Mrs Margaret Turner. In 1937 he was elected a Hunterian Trustee and continued to attend meetings at the Royal College of Surgeons of England, but his fellow-trustees could not help observing that although he retained his happy contented disposition he was finding his way about with difficulty for his sight was going and his step had lost its spring. About a year before his death he became completely blind and his muscles had lost their strength, but we who had the privilege of seeing him in a nursing home at Godstone, found in him the same gentle sense of humour, the same forgetfulness of self, the same happy spirit we had found in him in the days of his youth. He died in his sleep, his daughter being with him. A memorial service was held at St Peter's, Limpsfield, on 12 October 1953, cremation taking place on the same day at Croydon.

As has already been noted, Mr Burne was a fellow of the Zoological Society from 1890 to 1953—a period of 63 years; he was connected with the Royal College of Surgeons of England from 1892 to 1953—a period of 61 years. In 1912 he joined the Linnean Society serving on its Council for three terms and as its Vice-President in 1925-26. At one time he served the Malacological Society as its secretary. He worked for the Society for the Preservation of the Fauna of the Empire and for the Marine Biological Association as well as for the Museums Association.

ARTHUR KEITH.

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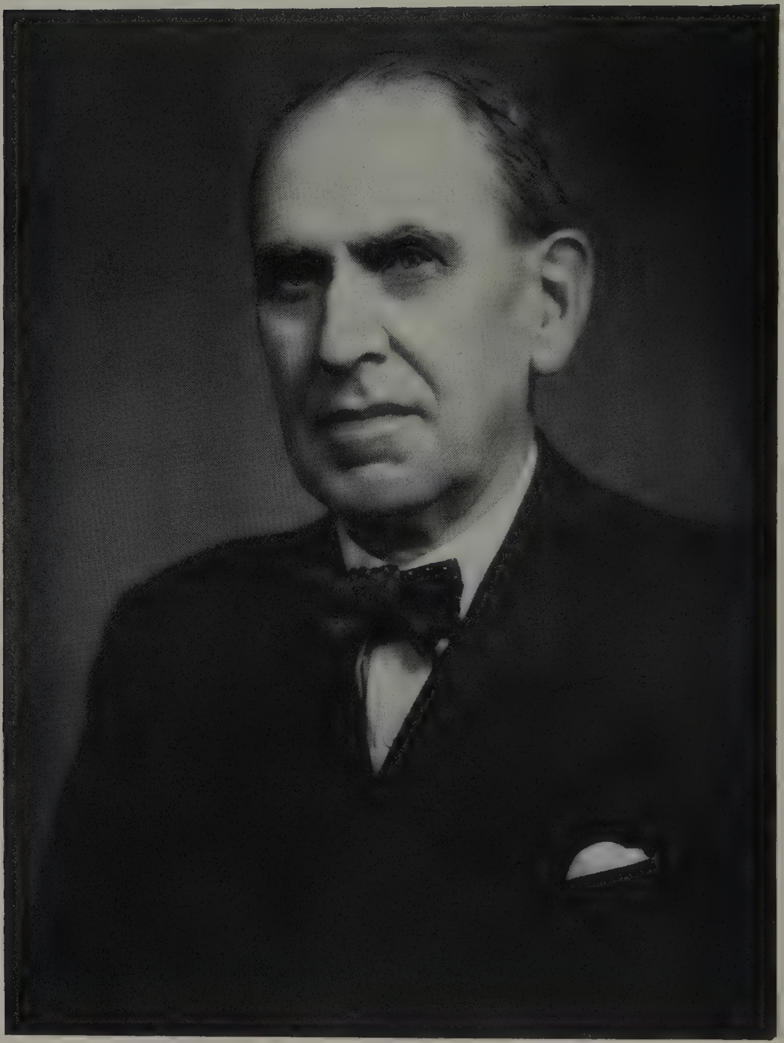

E. P. Cathcart.

EDWARD PROVAN CATHCART

1877-1954

Edward Provan Cathcart died at his home in Glasgow on 18 February 1954, seven years after his retiral from the Regius Chair of Physiology in the University of Glasgow. His death has removed a well-known personality in academic circles in Scotland, a scientist of international reputation devoted in the broadest sense of the term to the study of his fellow men, and a man whose human sympathies and understanding had an influence on his associates which can hardly be over-estimated.

E. P. Cathcart was born on 18 July 1877 in the town of Ayr. His forbears were practical people interested in agriculture and commerce, and in iron and steel. His father, Edward Moore Cathcart, was a merchant in the town of Ayr; his mother was the daughter of a rivet and bolt manufacturer whose home was at Invereck, near the village of Kilmun on the Firth of Clyde. His father died at the early age of 37, when Cathcart was only nine, and he with a younger brother and sister were brought up by the widowed mother. She, in addition to being a very good amateur painter who had gained several awards at the Glasgow School of Art, was in many ways a woman of intellectual attainment and personality. Cathcart's great affection and respect for her were obvious to all who came into close contact with him. Indeed, he was wont to acknowledge that, in his early life, he owed virtually everything to her self-denial and encouragement, and, during the many years that he worked in Glasgow, it was very rarely that he omitted from his daily routine a brief visit in the evening to his mother's flat. She lived to the age of eighty-nine.

Cathcart gained his school education at Ayr Academy, one of the oldest schools in Scotland. In the secondary division of this school, he was placed on the classical side, more, he records, because it was the customary trend of the school than from any special choice of his own. He frequently remarked that he had few pleasant memories of his school-days until his final school year; that he mixed little with the other scholars and took little or no part in games. To those who knew him in later life the first seems incongruous, since one of his most striking attributes in adult life was an approachability and a capacity for friendliness towards all with whom he came in contact. It is, however, true that though not uninterested in sport and athletics, his interest was always that of the onlooker and it was a matter of comment that, in spite of little personal participation in the customary forms of physical exercise, he yet retained until the last few years of a long life a most enviable degree of lean energy, vigour, and physical fitness.

Though he qualified for entrance to a Scottish University in 1894, Cathcart remained at Ayr Academy for an additional year, a year which he admitted he enjoyed and one which was the most influential of his scholastic period. During this year, in addition to the pursuit of English and of Modern Languages, one of the science masters of the school who, perhaps, had the perspicacity of a real 'careers master', gave him privately an intensive course in theoretical and practical inorganic chemistry. To this teacher Cathcart gave great tribute, not only for introducing him to the realms of science in general and chemistry in particular, but for teaching him 'how to work'.

Having entered Glasgow University in 1895, Cathcart graduated M.B., Ch.B., in 1900. His undergraduate career was good though not marked by outstanding brilliance except in the subjects of obstetrics and gynaecology and, for a short time during his immediate post-graduate career, he dallied with the idea of specializing in gynaecology. Any lack of sociability which he may have displayed at school must have disappeared with adolescence, for he took an active part in the administrative side of student activities, being a member of the Board of the Students' Union, a member of the executive of the Students' Representative Council and, in his final year, he was elected President of the University Medico-Chirurgical Society. The potentialities of an enquiring mind showed in two small investigations, carried out as an undergraduate in the Department of Forensic Medicine, on the guaiac test and on occupational callosities of the hands, some of the results of which were incorporated in Glaister's *Text-book of forensic medicine.*

The academic teachers who influenced him most were, however, Ralph Stockman who inculcated in Cathcart, as in many others, the spirit of scientific doubt; and Sir Robert Muir, who aroused in him an interest in bacteriology. The latter influence was the starting-point of Cathcart's scientific career since, in 1901, after a little over a year in hospital posts, he set off to Buchner's laboratory in Munich. At that time the Munich Institute of Hygiene was in the temporary charge of Martin Hahn and Hahn allowed Cathcart time off to attend the course in human metabolism by Carl Voit, by then an old but still very active and stimulating teacher. Later Hahn encouraged Cathcart to study chemical pathology under Salkowski in Berlin, where he spent a year partly in attendance on Salkowski's training course and partly on research. These experiences in Munich and Berlin and particularly the influence of Carl Voit, were undoubtedly the original stimuli for a career which, devoted in its earlier years to the study of protein metabolism, widened gradually to a consuming interest in the effect on man of his whole environment.

Returning to Britain in 1905, Cathcart secured a post first as Research Scholar and later as Junior Assistant under Hedin at the Lister Institute. The three years spent at the Lister he regarded as of great value to his subsequent development. He records how greatly he benefited from Hedin's tedious but salutary axiom that each experiment ought to have ten controls. His researches at the Lister provided the material through which he gained the M.D. of his own university with the highest honours. And, perhaps most significant of all, his appointment at the Lister brought him into association with J. B. Leathes whose work at that time on uric acid metabolism immediately aroused the sympathetic interest of one who had so recently come under the spell of Carl Voit.

Meantime, the importance of biochemistry as a special branch of physiology, already well appreciated in Germany, was gaining recognition in Britain. The first Chair of Biochemistry had already been instituted at Liverpool and, in Glasgow, a legacy from a former medical graduate had enabled the establishment in 1905 of the Grieve Lectureship in Physiological Chemistry. To this lectureship, Cathcart was appointed as the first incumbent, taking up duty on his return from the Lister in 1906. He remained in this post until 1915 when he was appointed Professor of Physiology in the University of London, attached to the London Hospital Medical School.

His period as Grieve Lecturer, almost a decade, was marked by the appearance of a large number of contributions, both personal and with collaborators, relating to various aspects of protein metabolism. It also included two periods of leave for foreign study, the first in 1908 when he spent five months with Pavlov in St Petersburg, and the second in 1912, when he was granted a year's leave to become a research assistant with F. G. Benedict at the Carnegie Institution of Washington in Boston. This period in Boston not only introduced him, through investigations into energy metabolism, to a new line of research but offered many opportunities of discussing with Otto Folin problems of mutual interest in protein metabolism.

Cathcart left Glasgow to take up his appointment at the London Hospital when the first World War had just entered upon its second year and, for the next four years, all but teaching duties gave way to war service. Commissioned originally in 1908 as an officer in the Glasgow University Officers Training Corps, of which he was for several years Commanding Officer of the Infantry Unit, Cathcart was transferred in 1915 to the R.A.M.C. for anti-gas duties, later becoming Deputy Director Anti-gas Services, Home Forces. In 1917, he was transferred to the personal staff of the Director-General Army Medical Services, with the rank of Lieutenant-Colonel, for special work in connexion with the feeding of the army.

His stay in London lasted for four and a half years. In 1919, through the generosity of William and Frederick Gardiner, shipowners in the city, Glasgow instituted the Gardiner Chair of Physiological Chemistry and Cathcart was invited to become the first incumbent of the chair. Nine years later when Nöel Paton resigned in 1928, Cathcart was translated to the Regius Chair of Physiology which he held until his retiral in 1947. His transfer to the Regius Chair was a natural and expected event. It introduced no conflict to interests which had for long been more physiological than biochemical.

The long period of Cathcart's association with Glasgow witnessed not only a phenomenal development in the sciences of physiology, biochemistry and nutrition, but also an impressive growth of his university. In both, Cathcart was an outstanding figure. His services to his university were manifold and diverse. In the administrative affairs of his university, he held many offices; indeed, it is difficult to think of any major development of his period in which he did not take a formal or informal part.

He represented his university on the General Medical Council from 1933 to 1945 and during his membership served on many of its committees, including the Executive Committee. When he resigned in 1945, the President said of him in his presidential address: 'We shall miss a most charming and engaging personality who, by his humanity of outlook and sense of humour, endeared himself to all his colleagues on the Council. Our proceedings in every department of our activities will be sadder for his absence.'

A real genius for friendship was one of Cathcart's greatest gifts and the geniality of his temperament and his kindly way with students made him much sought after as an external examiner. At one time or another, he acted in this capacity to the universities of Aberdeen, Cambridge, Durham, London, Oxford, St Andrews, and Wales, and also to the Royal College of Veterinary Surgeons.

Cathcart's abundant contributions to medical science, many arising from investigations carried out in laboratories abroad, early brought him an international reputation and there were few important centres of physiological teaching and research in Europe or America in which he was not personally known. He was elected to membership of the Physiological Society at the annual meeting of 1904. Among those elected at the same meeting were Dean, Plimmer, and Lewis, later Sir Thomas Lewis. He was also a member of the Biochemical Society. In 1922, he was President of the Physiology Section of the British Association when he delivered a presidential address on 'Physical fitness and the factors which influence it.' In 1931, he presided over the Pathology and Biochemistry Section of the British Medical Association when it held its annual meeting at Eastbourne. He was elected to the Royal Society in 1920 and served on the Council from 1930 to 1933. He became a Fellow of the Royal Society of Edinburgh in 1932, and, in 1948, one of the Society's Vice-Presidents. In 1937, under the auspices of the British Medical Association, he gave the Sir Charles Hastings lecture, choosing for his subject 'Food and nutrition' and delivering an address which admirably fulfilled the intentions of the founder. In 1940, he was invited by the Royal College of Physicians of London to give the Oliver-Sharpey lectures, the subject of these lectures being 'The mystery of alimentation'.

In 1939, Cathcart was honoured by election as an Honorary Member of the Canadian Medical Association and, in 1949, by election to the Honorary Fellowship of the Royal Faculty of Physicians and Surgeons of Glasgow. The University of St Andrews bestowed upon him an LL.D. in 1928, as also did his own University on his retiral.

Cathcart's special interest in the practical applications of his science, particularly in the sphere of nutrition, coupled with the soundness of his judgment, led to a demand for his services on many Government Committees. Most of these appointments were intimately associated with his researches and are best considered in that connexion. For his public services he was, in 1924, awarded the C.B.E. By honours, however, he set little store except in so far as they reflected the goodwill of his colleagues, and many of his intimate friends had shrewd suspicions that other honours were offered but declined.

In 1913, Cathcart married Gertrude Dorman Bostock, daughter of Henry Bostock and Alice Susannah Marson, Stafford. His wife was a graduate of Glasgow University both in science and in medicine, and had been a research worker in the laboratory of physiological chemistry for four years prior to the marriage. She was the author of a number of scientific papers and also of a *First book of physiology and hygiene* which ran through many editions. Cathcart's marriage brought him much happiness both in academic and in domestic life. His staff and research workers were almost as well known to Mrs Cathcart as they were to him and she was a kindly and sympathetic hostess and friend on the numerous occasions on which they were invited individually or in parties to the Cathcart home. There are three daughters of the marriage: Margeth Alice; Agnes Elizabeth Dorman; and Catherine Provan. All three qualified in medicine and one, the second, specialized in obstetrics and gynaecology. All are married, the eldest to a member of the dental profession, the second to a urologist, and the youngest to a District Commissioner in Kenya. All three have now young families.

Research

Cathcart's working life as personal investigator and also as a director of scientific investigation by others falls naturally into four main periods, each following as an evolutionary consequence of the preceding and together making a readily recognizable whole.

The first period comprised his early post-graduate work in Germany and in the Lister Institute. Though initially directed towards bacteriology, this period led, through the stimulus of Carl Voit and his association with Leathes, to a diversion of his interest to physiological chemistry and, in particular, to problems of protein metabolism. The second period was initiated by his work with Benedict in Boston, an experience which widened his interests to the consideration of the energy requirements of the body and of metabolic requirements in general.

The third period, which stemmed from these new interests, began with his investigations in 1917-18 and in the immediate post-war period into the energy requirements of the soldier. Coupled with his later membership of the Medical Research Council and especially of its Nutrition Committee and also his Chairmanship of the Industrial Health Research Board, his investigations on the soldier stimulated inquiries into the physique of civilian industrial workers and the dietary habits of diverse sections of the community.

In the fourth and last period, springing almost certainly from his chairmanship of the Scottish Health Services Committee, we find Cathcart's interests broadening still further to a consideration of the welfare of the community at large. This final period was marked by an interest less in nutrition as such than in the factors leading to good physique, and in education in general. With this summary as a general background, the details of his multifarious contributions as an investigator may perhaps be better appreciated.

Following upon a year or so of post-graduate hospital appointments, Cathcart spent the better part of two years in Germany where, under the tutelage of Hahn and Salkowski, he published papers on the reducing powers of bacteria and on the metabolism of glucosamine and chitosamine. The main influence of this period, however, was his attendance upon Carl Voit's course on human metabolism.

When he returned to Britain in 1903 to the Lister Institute, he produced a number of papers on bacteriological subjects and also an investigation into the anti-tryptic action of blood-serum. The latter he submitted for the Glasgow M.D. and was awarded the degree with honours and a Bellahouston Gold Medal for the special merit of his thesis. Of much greater significance on his subsequent career was his association at the Lister with Leathes who, interested initially in the diurnal variation in the output of uric acid, had already made a number of investigations into the effect of febrile conditions on the excretion of uric acid. In Cathcart whose enthusiasm for protein metabolism had already been fired by Voit, Leathes found a sympathetic and willing collaborator and together they pursued inquiries into the effect of artificial lowering and raising of the body-temperature on the excretion of this substance. Together also, they published an experimental investigation in animals which added evidence to the subsequently universally accepted view that proteins are absorbed as amino-acids.

On his return to Glasgow as Grieve Lecturer on Physiological Chemistry, Cathcart pursued his interest in protein metabolism by a starvation study upon Beaute, a 'Hungerkunstler'. Though by no means the first of such studies, his investigation of Beaute remains a classic for the special care devoted to the investigation not only during the starvation period but also in the pre-starvation régime and in the period of recovery. The accounts of this study, coupled with his investigations with Leathes on uric acid formed the substance of a thesis successfully submitted in 1908 for the D.Sc. Degree of his university.

Cathcart's investigations with Leathes and his own work on Beaute formed the first of a long series of inquiries, extending over the next fifteen years, into various aspects of protein metabolism and more especially, the effect on protein metabolism of altering the intensity of the concomitant fat and carbohydrate metabolism. In much of this work he acted both as investigator and as subject. It was a period of great enthusiasm and activity during which he attracted to his laboratory post-graduate workers from a variety of sources, many of whom have since attained eminence in their own fields. Among them were J. B. Orr (now Lord Boyd-Orr) who shared in some of the investigations of the effect of carbohydrates and fats on protein metabolism and Graham Brown who participated in researches devoted to creatine in relation to muscular activity, then an engrossing problem to many physiological chemists.

Cathcart's train of researches on protein metabolism was interrupted by two periods of overseas study. In 1908, he was granted leave of absence to work with Pavlov in St Petersburg. There he spent five months at a time when Pavlov had just begun his research work into conditioned reflexes, and Cathcart was thus fortunate to be in at the start of a new line of research which was to become of far-reaching importance. It is probable that this initiated the interest in behaviouristic psychology which Cathcart retained as a side-line for the rest of his life. His period with Pavlov also offered opportunities of animal operative technique of which he had previously comparatively little experience, and he was particularly proud of one of the statements in his testimonial from Pavlov which testified to his skill as an operator. When he left St Petersburg, Cathcart had still some weeks of leave in hand, and these he employed in an excursion to Moscow, Nijni-Novgorod, down the Volga to Astrakhan and thence to Baku, Tiflis, Batum, Constantinople, Smyrna, Athens, and Belgrade. Though it provided no professional experience, he set great store by the general educational value of this tour.

On his return to Glasgow, Cathcart utilized the Pavlov technique for investigations into the existence of a prepyloric sphincter in the stomach and of the reflux from intestine to stomach, and he published three reports on such problems. It was characteristic, however, that animal experimentation made no great appeal to him. Indeed, he was on occasion violently critical of what he considered the indiscriminate application of animal experience to human physiology; he was much more attracted by experiments on the 'whole man'. Thus, though the 'Pavlov dogs' were a great source of interest in the Glasgow laboratory at that time, the results they provided were only interludes in the main train of inquiry into protein metabolism which continued as Cathcart's chief interest.

In 1912, there appeared, in the well-known series of *Monographs on Bio-Chemistry*, the first edition of Cathcart's *Physiology of protein metabolism*. This was an encyclopaedia of the knowledge of protein metabolism then existing. On the fly-leaf of this monograph appears Occam's razor: 'Entia non sunt multiplicanda praeter necessitatem.' Cathcart boasted a life-long addiction to this axiom, an attitude of mind which, coupled with his scepticism about the validity of any but the most cautious application of the results of animal experiment to man, explained his highly critical attitude towards much of the earlier experimental work on the vitamins. In many respects, subsequent events justified his scepticism.

In 1912 Cathcart was given a second period of leave of absence to accept a Research Associateship at the Carnegie Institution in Boston. There he worked under F. G. Benedict. The immediate result of that year's work was the classic *Carnegie Institution Publication* of 1913 on *Muscular work: a metabolic study with special reference to the efficiency of the human body as a machine.* In addition to leading to the production of an outstanding contribution in this particular field of physiological knowledge, Cathcart's association with Benedict introduced him to the energy aspect of metabolism and thereby to an enhanced interest in the quantitative requirements of the body in general, an interest which coloured the whole of Cathcart's subsequent career.

Returning to Glasgow from the U.S.A. in 1913, Cathcart resumed his researches on protein metabolism while, at the same time, constructing apparatus designed on Benedict lines for the determination of the energy metabolism by closed-circuit methods. Before much progress had been made with this apparatus, he had left Glasgow to take up the Chair of Physiology at the London Hospital Medical School. In any event, the closed-circuit apparatus with its forced immobilization of the subject was ill-adapted to the type of energy-exchange investigation in which Cathcart later became so recognized an authority, and all his work on energy expenditure was done with the Douglas-Haldane technique.

Cathcart's four and a half years in London, apart from the teaching duties of his Chair, were entirely devoted to the service of his country in the war effort, first in the anti-gas services and later on the personal staff of the Director-General Army Medical Services for special work in connexion with the feeding of the army. In addition, he served as a member of the Food (War) Committee of the Royal Society, that group of physiologists, biochemists, agriculturists, and statisticians who advised the Government on the production, importation, and equitable distribution of food supplies when, by 1917, the loss of shipping had made the position of the country's food situation literally desperate.

Demobilization and a change of post came simultaneously for, in 1919, Cathcart returned to Glasgow to become the first Gardiner Professor of Physiological Chemistry. In the same year, he was appointed one of the two civilian members of the Army Hygiene Advisory Committee. This Committee dealt with the optimum load to be carried by the soldier, the improvement of his clothing including the now familiar 'battle-dress', the living conditions of barracks and, indeed, the soldier's environment in the broadest sense of the term. Cathcart was no mere adviser on this Committee; he was notably an investigator who furnished it with scientific data especially in questions of energy expenditure. To this end, during his first few years as Gardiner Professor, many investigations were carried out under his direction at Glasgow by specially detailed officers of the R.A.M.C. on the metabolic cost of marching and load-carrying under army conditions, using the Douglas bag technique. The results of these were published in a series of reports which appeared in the *Journal of the Royal Army Medical Corps*.

Parallel with this utilitarian research, Cathcart stimulated his university assistants and others who had come to work under him to carry out investigations of a similar but somewhat more academic character. In consequence, there appeared between 1919 and 1927 numerous papers in the *Journal of Physiology* and elsewhere on basal metabolism; on various aspects of muscular activity such as the energy cost of positive, negative, and static work; on the influence of speed of working on efficiency; on the energy costs of various everyday tasks; and on the energy cost of work on various dietary régimes. Almost the last of the investigations of this period were those carried out in 1927 with Markowitz on the immediate effect on the respiratory quotient of the ingestion of various sugars, an inquiry which emphasized incisively the danger of an oversimplified metabolic interpretation of this arithmetical ratio between carbon dioxide production and oxygen utilization.

Meantime, Cathcart had been appointed in 1924 a member of the Medical Research Council and a member of its Committee on Human Nutrition. Three years later, he was appointed a member, and shortly afterwards became Chairman, of the Industrial Fatigue (later Industrial Health) Research Board. With these two appointments may be observed a not unnatural shift in the focus of his interest from one directed towards metabolic inquiries in the individual to one centred on the physique and nutritional status of large sections of the community. In the following year, he succeeded Nöel Paton on the latter's retiral from the Regius Chair of Physiology. The translation was a perfectly natural one and, indeed, it would have been difficult to envisage any other sequence of events. His change of title involved little if any interruption in his main interests and the first few years of his tenure of the Regius Chair saw the fruits of his membership of the Medical Research Council and the Industrial Health Research Board in reports of investigations on the physique of the industrial worker and in a number of dietary studies.

Under the aegis of the Industrial Health Research Board, he directed three major investigations. The great influx of women into industry, brought about initially by the First World War but continuing into the post-war years, led the Industrial Fatigue Research Board to organize studies into various aspects of female labour. Cathcart, developing his previous work on the effect on the cost of movement of varying the position of the infantryman's pack, took a large share in this general inquiry by organizing and directing an interesting investigation by Bedale on the comparative cost in women of carrying loads in different positions. This work clearly demonstrated that the metabolic cost varied with the degree to which the body's normal centre of gravity was displaced.

This was followed in 1928 by the report of a second investigation, carried out with the assistance of Bedale, Weatherhead and others on the 'Physique of women in industry'. This, an inquiry conducted at the request of the

Home Office, involved the collection of anthropometric data relating to some 4000 women in different occupations. For each individual, details were recorded of height, weight, strength of grip and lifting power, the particular object being to determine the load that can be carried or lifted without injury or discomfort by women workers under industrial conditions.

Seven years later appeared a report of the third major investigation. A companion inquiry to the second, this was carried out on men in industry by Cathcart, Hughes and Chalmers. It was, however, on a much more extensive scale, covered nearly 14000 individuals in various occupations and included for comparison a sample of over 1000 unemployed and nearly 2000 students. The data collected were the same as for the women but they were much more extensively analysed with regard to changes with advancing age, geographical situation, occupational variations, and an interesting comparison was made between employed and unemployed.

Cathcart's association with the Medical Research Council and its Nutrition Committee led directly or indirectly to the publication, between 1924 and 1940, of no fewer than five dietary studies carried out with Mrs Murray and others under his direction. The first, in which he was associated with Paton and Greenwood, arose from various rumours and public assertions of starvation in the mining industry. It was an attempt to assess the state of nutrition of the families of miners during the period of serious industrial depression in 1922/23. Family budgets were investigated in five different mining areas, four in England and one in Scotland. The report brought to light many interesting facts concerning the calorie consumption in the different areas, its relation to income, and the efficiency with which the family income was expended on food. The investigation also included an inquiry into the heights and weights of miners' children in comparison with other children of the same area. The non-existence at that time of any widespread provision of advice on dietetics is commented upon and the report urges that housewives would be greatly helped to secure a more adequate return for their expenditure on food by a better dissemination of knowledge, both of the economic and the nutritional aspects of diet.

The second report, published in 1931, describes the results of an enquiry into the diets of 154 families in St Andrews. St Andrews was chosen because it offered (*a*) a small population of widely different trades and professions which were representative of all economic levels within the community, and (*b*) the facilities and resources of the James Mackenzie Institute to assist the inquiry.

From this investigation there emerged much of interest in the relation of diet to income and social class; on the relation of the family diet to the efficiency and personal character of the parents; and on the physical condition of the children in the various groups as assessed by height and weight. There was added to the report a most valuable appendix wherein is discussed the validity of the somewhat arbitrary values then commonly used for 'family coefficients' in the light of the real facts learned by the investigators concerning the proportion of the family food actually consumed by each member. The ration cards of the Second World War perhaps owed their generosity to the juveniles to this inquiry.

The interest of the results of the St Andrews investigation fired Cathcart with the ambition to build up a survey of the dietary habits of the whole country through a series of studies on a number of communities of divergent environment and occupation. It was an ambitious aim which was never completely achieved but its pursuit led to three further dietary investigations.

In 1932 appeared an inquiry into the diets of working-class families in Cardiff and Reading. In contrast to the St Andrews investigation where the aim was to cover all social levels, the Cardiff and Reading inquiries were designed to secure data relating to two working-class communities who were living under divergent industrial and environmental conditions.

The fourth report, which appeared in 1936, was a re-analysis of the data of the St Andrews, Cardiff and Reading inquiries from a new standpoint. In the former reports attention had been focused on the food consumption in terms of the gross amounts of protein, fat, and carbohydrate eaten, and little attention had been given to the sources from which these were obtained. As so analyzed, the data had shown remarkable uniformity in the percentage of total calories derived from each of these proximate principles. This fourth report re-analyzed the data in terms of the actual purchased foodstuffs from which these proximate principles had been obtained, with the result that the types of foodstuff purchased were found to vary widely in the three different communities. It was thus clear from the analysis that the uniformity of the proportional representation of protein, fat, and carbohydrate in the total calorie consumption existed quite independently of the type of foodstuff and, in the words of the report, 'seemed to represent the operation of some kind of instinct'. In this report there is re-iterated with even greater emphasis the plea of the 1924 report for educating the housewife in elementary dietetics by demonstrations in which the methods used were those available in her own home.

The fifth and last report was a study of the diets of families in the Highlands and Islands of Scotland. Here the aim was to procure data from purely rural areas remote from large shopping centres. These were analyzed both from the standpoint of calories and proximate principles and also with regard to the actual foodstuffs consumed.

An interesting side-light arising from these inquiries was the finding that the total calories of purchased food lost by the housewife as refuse and waste was very much less than the arbitrary 10 per cent which had been the conventional allowance in dietary studies.

Cathcart's wide interests led to a demand for his counsel not only in the domain of physiology and nutrition but also on bodies dealing with agriculture and the production of food. From 1926 to 1930 he was a member of the Development Commission's Advisory Committee on Agricultural Science and for ten years he sat on the Agricultural Research Council. In 1932 he was a member of the Cattle Diseases Committee of the Economic Advisory Council.

His most intimate connexion with agricultural investigation, however, was one closely associated with his own university and incidentally with the neighbourhood in which he had spent his childhood. In 1928, following upon recommendations made by the Development Commissioners that there should be established in Scotland an Institute for Dairy Research, there came into being the Hannah Dairy Research Institute. During the first two years of its existence, while awaiting the completion of its own buildings near Ayr, the then somewhat embryonic Institute had its base in the University Department of Physiology and Cathcart acted as its interim Director. Later when the Institute had moved to its permanent premises and Dr Norman C. Wright had been appointed Director, Cathcart continued to retain a very close interest in the Institute, becoming Vice-Chairman of its governing Council, a post which he held until his retiral from the University in 1947.

Throughout Cathcart's tenure of the Regius Chair of Physiology, his services as an adviser were increasingly sought and committee work became an ever-growing drain on his time. Under the Health Section of the League of Nations, he served as a member of the Technical Commission on Nutrition and as Chairman of the Committee on Family Co-efficients. He was also a member of the International Labour Office's Advisory Committee on the Nutrition of Workers.

As a counsellor, however, probably his most important service and one which had the greatest influence on his later career was his appointment in 1933, first as a member, and very shortly thereafter as Chairman, of the Scottish Health Services Committee, a Committee which is still frequently referred to as the 'Cathcart Committee'. In the course of its three years' deliberations, this Committee made a most comprehensive review, with regard to Scotland, of population growth and distribution, of social and economic conditions, of the then state of health of the people, and of the health services existing at that time. The report concluded with recommendations as to the general lines along which future health policy should be directed. Having laid great stress on the fundamental importance of the general practitioner as a 'family doctor', the Committee recommended an extension of the National Health Insurance scheme to the dependants of insured persons and to others. The Committee also recommended the recognition of the voluntary hospitals as an integral part of the Health Service of the country, with a measure of general supervision and direction by the central Health Department, coupled with the preservation for each hospital of autonomy in its own internal management. There is no doubt that this Report provided a vast amount of useful information in the framing of the Health Service of today. Indeed, in the White Paper (1944) which presented to Parliament the proposals for a National Health Service we find the following: 'The Cathcart Report is too comprehensive in scope to lend itself to brief quotation but it is one of the most complete official surveys of the country's health services and health problems yet attempted. The recommendations of the Committee have already formed the basis of legislation in particular fields.'

Cathcart's direction of this Committee had a natural influence in broadening still further the interests of his later career. An interest which had been previously focused mainly on nutrition and diet was extended now to embrace the whole field of the general health of the people and, in particular, their physical fitness and education. Reference to the bibliography will show that many of his publications from 1938 onwards concern the assessment and promotion of physical fitness in its widest sense. The same trend is to be seen in his administrative interests. In 1937, he was appointed a member of the Scottish National Advisory Council on Physical Training and, in 1942, a member of the Scottish Medical Advisory Council on Education. In 1943 he was appointed Chairman of the Rehabilitation Committee of the Scottish Medical Advisory Committee.

By this time, Cathcart was within sight of his retirement from academic life and committee work was absorbing the greater part of his time. Nevertheless a notable feature of his career and one fully appreciated by his university staff, was that he never allowed his extra-mural commitments as a national counsellor to interfere with what he regarded as a sacred duty, his work as a teacher and as an administrator within his own university department. It was inevitable, therefore, that personal research should suffer and the last few years of his tenure of the Regius Chair were unmarked by contributions other than reviews and re-analyses of past work, with one rather singular exception. In the last year or two of his university service he returned to pure laboratory research in the domain of histology and his last paper, published in 1947, was a histological study of the innervation of the nipple.

On his retiral under the age limit from the Regius Chair, the Department of Health invited him to become the first Chairman of the Western Regional Hospital Board under the Scottish Health Service. In view of his previous direction of the Scottish Health Services Committee and his wide administrative experience, no better choice could have been made and the appointment was acclaimed with general satisfaction. Cathcart accepted this invitation to the pleasure of all concerned but a period of acute ill-health unfortunately forced him to resign the charge before the 'appointed day', and he spent the remaining six years of his retiral quietly at home, sustained by a lively interest in family developments, in old and new friendships, and by the enjoyment of ample leisure for reading.

Personal characteristics

Cathcart was a living refutation of the view that physiology is a purely academic discipline unduly concerned with the behaviour of the lower vertebrates. He was concerned with man in all his activities. Hence his interest in psychology, in philosophy, in education, and in the liberal arts. In his early days he was one of a small band which kept alive a remarkably successful repertory theatre in Glasgow. He was interested in music, particularly string quartets, in painting and in architecture. He had a most catholic taste in literature.

Such a rich personality could not fail to have a major impact on the life of his university. His lectures, illumined by personal anecdotes drawn from wide and varied experience and travel, were a delight and a stimulus to his audience. His students received a liberal education in subjects often far removed from physiology, while his generous sympathy with them in their academic work and his vital interest in their activities outside the classroom secured for him their universal affection and admiration.

Those who had the good fortune to serve an apprenticeship in research under him will always remember first the prodigious energy of the man. Body and mind were equally dynamic. The tall, big-boned frame, the characteristic abrupt movements, the changing expressions of the strong mobile features and the ranging tones of the deep Ayrshire voice all contributed to the impression of a personality as unselfconsciously forceful as it was honest and essentially humble-minded.

Tea-time in his department was very often the occasion of an all-round discussion of anything under the sun, and one could wish there had been a Boswell among his juniors. Often he would charge into a discussion from half-a-dozen different angles with an inconsequence which left less agile minds gasping at an illogicality which was only superficial. For he saw things whole and in the round and was impatient of the fine-drawn argument which left out relevant facts and what he used to call 'the all-important imponderabilia'. His warm approvals and sweeping disapprovals, frank enthusiasms and sudden caustic satire drove the discussion in every direction. Many of his apparently fantastic prejudices were undoubtedly thrust at his juniors as a challenge to them to justify their own opinions.

Especially he loved to discuss human behaviour, people's attitude to their work, their hobbies and their foibles. He was genuinely interested in whatever interested other people, even when it was games, for which he had no time himself, saying apologetically: 'My only hobby is talking.'

From his first year as a student to the last days of his life, Cathcart read all manner of books, excepting perhaps fashionable trivialities. He found particular delight in history and in the lore of old stained glass. He was in general impatient of politics, somewhat sceptical of social reform and humorously astringent towards idealistic theories. It was his passion for fact-finding which led him into lines of work which in the end made him a notable contributor to state-guided social improvements. For himself, he maintained that the individual was responsible for his own development; but he apparently saw no inconsistency in putting himself to great pains to encourage, goad, and materially help a large number of individuals to exert themselves to further effort and study. He could be grim and gloomy occasionally, but his mordant condemnations were reserved wholly for lazy or dishonest work.

Himself a regular attender at divine worship, Cathcart was deeply interested in the variety of people's religious beliefs and practices. In this, as in all his interests, his wife's mind and his own worked together in co-ordinated opposition like a well-sharpened pair of scissors, and to hear their discussions was always a delightful tonic. Their hospitality kept its legendary distinction even through the years when few people found time for converse with their friends.

Professor Cathcart was, in fact, a 'character' in the true succession of characters thrown up from time to time in the long history of the Scottish universities. But whereas most of these worthies have been distinguished by self-centredness, eccentricity or even acute quarrelsomeness, Cathcart will be remembered for the breadth and balance of his personality, peculiar only in being more than life-size in his appetite for work and in his intense enjoyment of the human scene.

For help in the preparation of this notice, I am indebted to Professor R. C. Garry and to my wife, both of whom, like myself, had the great privilege of working with Professor E. P. Cathcart.

GEORGE M. WISHART.

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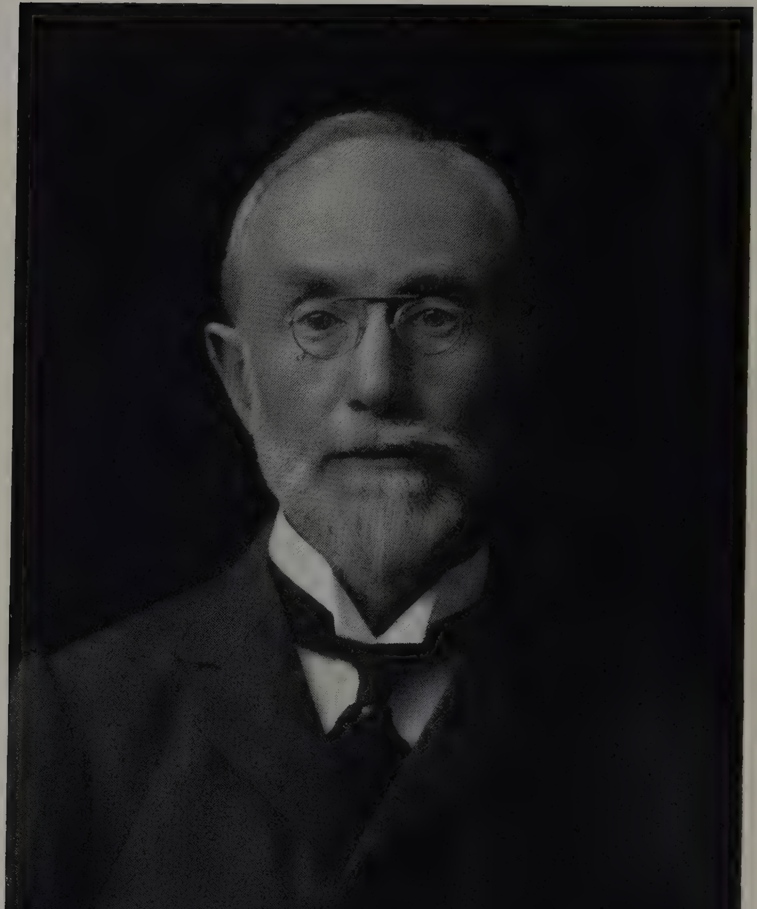

C. Dampier-Whetham

W. C. Dampier

WILLIAM CECIL DAMPIER

1867-1952

Sir WILLIAM DAMPIER was born in 1867 and named William Cecil Dampier Whetham. His early work was published under that name but later he changed his surname to that of his mother's family.

At the beginning of the seventeenth century the Whetham family were small landowners in Dorset, but in the nineteenth century Dampier's grandfather moved to London and became an important figure in its business life. He was knighted and became Lord Mayor of London. Dampier records that he acted as his grandfather's page on state occasions. Dampier's mother came from a Somerset family, one branch of which produced the famous explorer William Dampier. In early youth poor health prevented him from going to a public school, but in 1886 he went up to Trinity College, Cambridge. Here he came under the influence of J. J. Thomson who inspired him with a desire to undertake research in physics. After taking first class degrees in both parts of the Natural Sciences Tripos he started research work in 1889 at the Cavendish Laboratory. His first work was directed to finding out whether there is any slipping at the surface of a tube when a fluid passes through it. It had been thought that fluids which do not wet glass might slip, whereas those which do wet it would not. Dampier showed conclusively that there is no slipping. He next turned to the measurement of the velocity of ions in electrolytic solutions and devised an ingenious method in which direct measurements were made using a coloured solution. These measurements confirmed previous theories put forward by Hittorf and Kohlrausch. These researches led in 1891 to his being elected a fellow of Trinity. He remained a fellow of the college during the whole of the rest of his life.

About the time Dampier published his first work, Arrhenius had just put forward his theory of the connexion between the conductivity of dilute solutions and the corresponding depression of their freezing points. His theory did not receive immediate acceptance, and Dampier with E. H. Griffiths set to work to test it experimentally. They made accurate measurements both of conductivity and depression of the freezing point. Arrhenius's theory was shown to be in accordance with experiment. As a result of these studies Dampier published in 1902 his book on *The theory of solution*. This was for some time the standard textbook on the subject.

In 1897 Dampier married Catherine Durning Holt, daughter of Robert Holt a member of a prominent family of Liverpool shipowners. He had five daughters of whom two, Mrs Bruce Anderson and Miss Edith Whetham, became scientific research workers and university teachers.

In 1895 Dampier became a college lecturer and in 1907 a tutor. The duties of a college lecturer allowed some time for research work and between 1895 and 1908 he published sixteen papers, articles or books mainly on electrolysis and solution.

After Dampier had become a tutor his college duties absorbed an ever increasing amount of his time and he had to drop his laboratory work.

Dampier had always been interested in his own family history and his wife shared that interest. Their joint researches in this field gave rise to a broader interest in heredity and genetics. From 1903 onwards they published a number of notes which in 1909 culminated in their book *The family and the nation*. After describing in a popular way the state of the science of genetics as it existed in 1909 they discuss first the rise and decline of individual families. Then they trace effects of selective birth rates and what is now known as the Welfare State on the character of the nation. They believed that if only people's eyes were opened to the disastrous effects of a wrongly directed selective birth rate they would take measures to encourage the reproduction of good stock and the checking of evil strains.

With the dropping of his laboratory work Dampier applied his talent for clear writing to describing the work of others. In 1909 appeared the first edition of his *Recent developments in physical science*, a work which revealed an interest in physics extending far beyond the field of his own researches. This led him to think about the philosophy and history of science and he employed such leisure as his college duties allowed in collecting material for his *History of science* which appeared in 1929. This book went through many editions and was translated into many languages.

Since his earliest years Dampier had been attracted to country life. During his time as undergraduate and later when he was a college lecturer he gave as much time as he could spare to country sports. In 1909 he and his wife bought an old manor house and some farm land in Devonshire. This turned his mind in the direction of agricultural economics on which he published some notes which attracted attention. When the 1914 war began it was natural, therefore, that he should be called upon to help in a food production campaign. While he was so engaged he inherited in 1917 a small estate near Yeovil, and as soon as he could he put his agricultural ideas to the test by taking over two of his farms and operating them through a manager. At that time there was a great scarcity of cheese so he began a factory for making it on his farms. The surplus whey which this operation made available was used by Dr Leonard Harding in developing a new method for making lactose which ultimately developed into a commercial process.

When the 1914-18 war was over the fall in agricultural prices encouraged Dampier to investigate the relationship between money, prices and agricultural profits. On these subjects he published his opinions in the *Economic Journal* and elsewhere. He also published a small book, *Politics and the land*.

These publications brought him a number of posts in various forms of public service connected with agriculture. In particular his great service in connexion with the Agricultural Research Council and the Development Commission is recorded in the attached note by Mr E. H. E. Havelock the secretary of the Commission.

Dampier was elected a Fellow of the Royal Society in 1901 and was knighted in 1931. His loss is deeply felt in Trinity College to which he gave so many years of devoted service.

G. I. TAYLOR

NOTE BY MR E. H. E. HAVELOCK

As first Secretary of the Agricultural Research Council from 1931-1935 Dampier made a contribution to the firm establishment of the Council which was of exceptional value at that stage and was lasting in its influence. In so doing he rendered most distinguished public service. He was outstandingly well fitted by experience, width of interest, and qualities of mind and character for a task which was bound to be of some delicacy, for the new Council, unlike its sister organizations, the Department of Scientific and Industrial Research and the Medical Research Council, found already in existence a firmly rooted research organization and an administrative machine, which to those not familiar with it might seem already complex. This research organization, mainly consisting of grant-aided Institutes, founded largely on a scientific subject but sometimes on a sectional and industrial basis, had its origin in a scheme framed by the Development Commissioners in 1911, in co-operation with the Agricultural Departments, at a time when State aid for agricultural research was trifling in amount. Each Institute, except those few, such as the Veterinary and Plant Pathology Laboratories of the Ministry of Agriculture and Fisheries, whose duties arose out of legal obligations, was either attached to a University or had its own Governing body; they were in no sense State Institutions, nor were the members of their staffs Government servants. Nevertheless they depended almost entirely on State grants for their development and maintenance and these grants were borne on the Votes of the Departments of Agriculture, the source of the money being the Development Fund, from which advances are approved by the Treasury on the recommendation of the Development Commissioners. It was natural therefore that the wide powers granted by Royal Charter to the new Research Council, responsible only to a Committee of the Privy Council under the Chairmanship of the Lord President, should be closely defined in a Note on Administrative Arrangements, which preserved the responsibilities of the Departments and Organizations already concerned, while giving to the Council important functions of scientific influence and advice. The sum placed at the Council's direct disposal for research grants was at this stage quite small. It was therefore important that the Council's first Secretary should have gifts of diplomacy, wisdom and independence of outlook as well as an acknowledged scientific and academic status, and that, if possible, he should have too knowledge of agriculture and its problems. The Council were, indeed, fortunate in persuading Dampier to serve them for a period, nominally not on a full-time basis, though he gave them ungrudgingly of his time and thought, to the detriment of his health. He was completely disinterested, not only in the personal sense but because he was not entering this new sphere as a career and had no need to seek power or distinction.

Dampier saw immediately that the first task of the Council must be to make itself thoroughly familiar with the officers of the interested Departments and with the staffs of the Research Institutes, their work and their needs, even though the financial circumstances of 1931 were peculiarly unfavourable to development. His task was eased by the close links which were forged with the Development Commission, whose Chairman, Lord Richard Cavendish, became first Chairman of the Agricultural Research Council, and one of whose members, Sir Thomas Middleton, not only brought to the council table his unrivalled experience of the organization for agricultural education and research but was available in the office for consultation. The new Council shared offices and staff with the Commission.

A careful survey, lasting necessarily over two or three years, was therefore made through committees and sub-committees of the Council of all the institutes and centres of research, and at the same time the Council decided that it was in the field of animal diseases research that the need for strengthening all means of investigation was strongest. The Council therefore established a series of technical committees to examine the state of knowledge of specific diseases and subsequently to recommend what further action was needed. At the same time special studentships in animal health were created and plans were examined for extending and re-organizing the Royal Veterinary College in London and for bringing into closer association in Scotland the research activities of the Royal (Dick) Veterinary College and the Animal Diseases Research Association, as well as for strengthening other centres of investigation elsewhere. Dampier's own training and experience naturally led to his taking a very special interest in securing a closer association between teaching and research.

In the course of the Council's survey some organizational problems of difficulty inevitably arose, and it happened that one of the fields where this occurred was that of agricultural engineering, with which Dampier was already familiar as Chairman of the Ministry's Machinery Committees. His special interest in grassland farming led to his very active concern with problems of grass drying and he was chairman of the Council's committee which explored the technical and practical problems involved. Again, his long academic experience and his breadth of outlook were of the greatest service in negotiations with the University of Cambridge, which led to the setting up of a syndicate to consider how best the Cambridge Agricultural Research Institutes, which depended upon special Government grant-aid, could be brought into the closest relationships with each other and with the science departments of the University.

It was, in particular, a majority recommendation of one of the Council's technical committees on animal diseases, that on contagious abortion, in favour of the committee having an experimental herd directly under their control, which made clear the need for a wider interpretation of the Council's powers and functions. The intricate and necessarily prolonged negotiations which resulted in a Second Note on Administrative Arrangements being approved were conducted by Dampier with great skill and understanding, so that a decision was reached by general agreement which gave to the Council formally the right themselves to carry out agricultural research and employ scientific and other staffs for this purpose 'at research centres under their own direction, or elsewhere', while fully recognizing the need for obtaining departmental agreement if research under the Council were to be initiated at an institute receiving grant aid from one of the agricultural departments. With this definite acceptance of the Council's freedom to engage directly in research the establishment of their Field Station at Compton for controlled experiments on diseases, especially contagious abortion, requiring the use of considerable numbers of farm animals, became a possibility and Dampier threw himself enthusiastically into the initial search for a suitable site. At the same time the new arrangements emphasized the Council's status as the central guiding authority for all agricultural research.

Dampier's period of service as secretary was therefore rounded off by an advance in the Council's authority and freedom of action which was to lead to further developments of outstanding importance and which enabled the Council to establish immediately the case for a full-time scientific secretary and to appoint to this post one of their number, the late Sir Edwin Butler. Nor had this achievement been accomplished at the expense of any neglect of the Council's scientific examination of the position in the many branches of agricultural research covered by the existing organization or of plans for consolidating and extending that organization. The debt which the Council owed to Dampier's integrity, devotion and wisdom was, indeed, great, and it was at once recognized on his resignation as secretary by his appointment as a member of the Council. As a member he held office for ten years and it was characteristic of him that though ever ready to serve as chairman of a committee, such as that on dairying, or in any other way, he never once used unduly the authority or influence of his former position or stepped outside the role of normal member.

In 1933 Dampier was appointed a Development Commissioner, while still Secretary of the Agricultural Research Council, and he remained a member until January 1951, when his health, after a period of recovery, really began to fail. For the Commission's work, which covered so many aspects of country life and was concerned with policies of development passing beyond the research stage, Dampier had a great liking and affection and his retirement was a severe blow to him. He had taken a particular interest in the affairs of the Rural Industries Bureau, of which he was a trustee and during the war acting chairman of its committee, and in the progress of Rural Community Councils, representative of all the statutory and voluntary bodies in a county. He was chairman of the Cambridgeshire Rural Community Council. To his colleagues on the Commission his retirement came as a grievous blow, for he had in a very special measure their admiration and regard.

Dampier received in 1936 the Gold Medal of the Royal Agricultural Society for services to agriculture. He became a Vice-President of the Society.

Dampier faced every difficulty and disability with stoical courage and dignity, but also with wit and good humour. He was fastidious in his standards and deliberately restrained in expression, but those who were fortunate in winning his esteem had gained a most generous, loyal and affectionate friend. To work with him was to be accepted as a partner.

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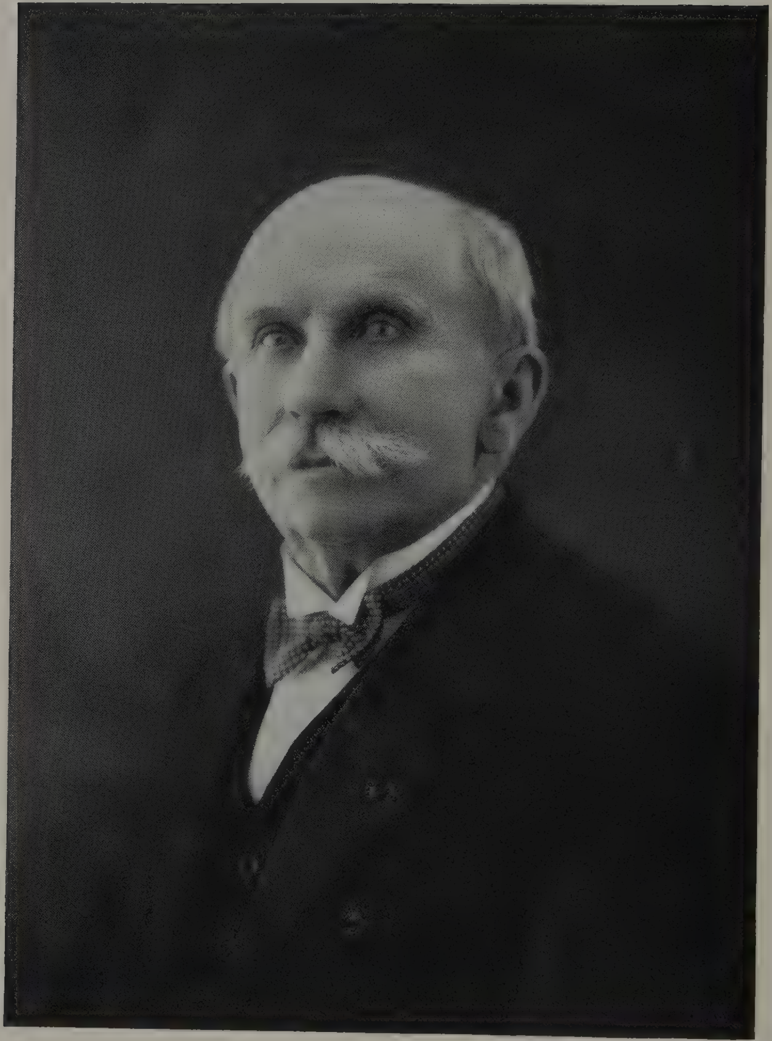

H. Deslandres

HENRI ALEXANDRE DESLANDRES

1853-1948

Henri Alexandre Deslandres was born in Paris on 24 July 1853. Destined for an Army career he was educated at the Ecole Polytechnique from 1872 to 1874. He secured rapid promotion and in 1879 he became Captain in the Engineers and qualified for staff appointments. But a scientific career had begun to appeal to him so strongly that in 1881 he retired from the Army and devoted himself to scientific research. Attached to the physics laboratory of the Ecole Polytechnique from 1883 to 1887 he published his first paper in 1885 on *Relations entre le spectre ultra-violet de la vapeur d'eau et les bandes telluriques A, B, α du spectre solaire.* In 1887 he joined M. Lippmann in the physics laboratory of the Faculty of Sciences of the Sorbonne. He obtained the degree of Doctor of Science in 1888 with a thesis on band spectra.

He first made his name known by the laws that he established governing the frequencies of lines in the bands in molecular spectra. When he started all that was known was the analysis of the green band of carbon monoxide by Piazzi Smyth and Alexander Herschel's links between the wave-numbers of the separate lines. The researches of Deslandres were stated by Kayser to have put our knowledge of band spectra on a totally different plane. Deslandres' First Law is that the intervals between the wave-numbers of successive lines of a band are in arithmetical progression. This he first found for each band in the spectrum of nitrogen near the negative pole in tubes at ordinary pressure. He analyzed other bands of nitrogen into series for which the same law held and extended the law to bands of cyanogen, oxygen and water vapour. The law can be expressed in the simple formula for the frequencies of lines in a series

$$\nu = Am^2 + a$$

where A and a are constants for the series and m takes successive integral values. He extended his law to cover bands of doublets, triplets, etc., repetitions of groups of lines similarly related.

Deslandres' Third Law

$$\nu = Am^2 + Bn^2 + C$$

where A, B, C are constant and m, n are integers followed from his further discovery that the heads of different series in bands of the same molecule are related in just the same way as the lines in the series from each head, though with different constants in the formula. Next Deslandres found that he could group the series of bands from different heads so that the squares of the constant C also gave intervals, which are in arithmetical progression; thus he could ultimately give the complete formula for the frequencies of the lines in the bands of a molecule in terms of three integral parameters m, n, p, and two constants B, C, in the form

$$\nu = f(n^2, p^2)m^2 + Bn^2 + \phi(p^2)$$

Deslandres never lost his interest in molecular spectra. One of his long continued researches was on the anomalous effects on band spectra obtained in the presence of magnetic fields. He designed instruments to give fields up to 63000 gauss. Towards the end of a long life he returned to the consideration from high dispersion spectra of perturbations affecting the arithmetic series in his laws and to the relations of molecular spectra with the constitution of the molecules. He describes his last paper presented to the Académie des Sciences eight days before his ninety-fourth birthday, as follows:

> 'La dernière de la longue série de Notes qui depuis 1929 sont consacrées aux relations simples qui unissent la composition chimique exacte de la molécule à son spectre Raman et qui ont conduit aux quatre lois des fréquences moléculaires . . .'

and adds:

> 'L'étude des molécules est actuellement abandonnée parceque les données expérimentales sont trop souvent insuffisantes. Mais leur étude sera reprise lorsque les fréquences Raman seront mesurées avec une précision plus grande et lorsque l'étude chimique des protides et des protéides aura fait des progrès notables.'

Brave words to be written at the age of 93. This work was never renewed by the old warrior, but his enthusiasm and faith in his ideas endured to the last. Before passing to an account of his career as an astrophysicist, mention may be made of his work as Colonel in the Engineers during the war of 1914-18. As commandant of the technical section of the Engineers in Paris he developed with Colonel Jouhandeau a weapon of short or medium range for incendiary and explosive shells described as a 'canon d'accompagnement'. This weapon was widely distributed in the French Army.

Deslandres left the Sorbonne in 1889 on being appointed by Admiral Mouchez as assistant astronomer at the Paris observatory. He was placed in charge of spectroscopic work, making use of the Foucault siderostat and adapting the great telescope of the observatory for its new line of work. With this instrument he studied at first radial velocities of stars but he broke fresh ground in measuring spectroscopically the velocity of rotation of Jupiter, and of Saturn and its rings—confirming Maxwell's theoretical views of their structure. He showed that the rotation of Uranus was retrograde like the revolution of its satellites. He also started spectroscopic work on the solar chromosphere and prominences, finding in the latter more ultra-violet lines of the Balmer series and fresh lines of helium; he completed our knowledge of the spectrum of neutral helium in the range then accessible to study.

It was this work on solar physics which was to place him in a leading position in the astronomical world. Simultaneously with Hale he attacked the problem of replacing by photographic methods the laborious visual study of the forms of prominences. He and Hale both discovered in 1891 the bright central reversal of H and K on the Sun's disk, and Deslandres developed his 'spectro-enregistreur des vitesses', an instrument differing from Hale's spectroheliograph in its wider second slit and discontinuous motion. This instrument has the advantage over the spectroheliograph that it enables measures to be made of the velocities in the line of sight of the gases, that give the bright reversals in the absorption lines. It does not give the beautiful smooth pictures that the spectroheliograph gives, but Deslandres showed in volume 4 of the Meudon *Annales* how successful he was in the design and use of both these instruments. Actually he was held back from completing his instruments till 1893 by lack of funds and by an expedition that he led to Fundioum in Senegal for a total solar eclipse on 16 April 1893.

Deslandres led three other eclipse expeditions—to Yezo for 9 August 1896, to Spain for 28 May 1900 and for 30 August 1905. Only at the eclipses of 1893 and 1900 did he have favourable weather conditions. He extended the known spectra of the chromosphere and corona into the ultra-violet and the infra-red, attempted to measure the rotation of the corona and determined the total radiation from the corona.

In 1898 he was appointed astronomer at the Astrophysical Observatory at Meudon under Janssen. He became Assistant Director in 1906 and Director in 1907. He greatly improved the equipment of the observatory, adding a first-class workshop, new spectrographs and coelostats and additional optical instruments of the highest quality. From 1908 onwards, with the help of Dr d'Azambuja, he produced beautiful results in his photographs of the Sun's surface in different wave-lengths and in different parts of a given line in the spectrum. The high quality of the Meudon instruments and the careful and accurate work of the observers is well seen in volume 4 of the *Annales de l'Observatoire de Meudon* (1910).

In this volume he gave a detailed account of the instrumental equipment of the Observatory and published a number of photographs of the Sun's disk in successive layers of hydrogen, calcium and iron gases, especially the upper layers. He studied the radial velocities over the bright faculae and of the dark filaments. He noted the disappearance of spots in the uppermost layers and the increased frequency of dark markings; in the highest layers these were not only more numerous but also darker and more extended. He detected a veritable reseau of dark markings on the Sun's surface and a systematic circulation of gases, which descended over the bright faculae and rose over the darker regions, especially over the filaments. He noted that the bases of prominences were joined to the dark alinements and filaments of the upper surface. In the regions of high latitude he found that the filaments were linked with coronal jets and streamers.

Deslandres also examined the relation between the sunspot cycle and terrestrial magnetism and he stimulated an international scheme for the continuous observation of solar phenomena from observatories the world over. From the rotation of the dark filaments regarded as helices with axes parallel to the lines of force of the Sun's magnetic field he deduced a value for the field of 10^{-7} gauss. He formed a model of the chromosphere in alternate layers with electric fields and magnetic fields changing sign from layer to layer. He also put forward the view that there were special disturbed regions of the Sun's surface, points of corpuscular emission arranged regularly over the Sun's surface and working at different depths. The consequent 27-day period in magnetic disturbances is well established, but not the regular distribution of the disturbed areas over the Sun's surface. These various suggestions formed part of a theory for the circulation of the Sun's atmosphere. He suggested in 1902 the existence of Hertzian radiation from solar prominences, forty years before the first observational evidence of such radiation was secured. Other work carried out by Deslandres at Meudon included the discovery of reversals in the H and K lines in spectra of high dispersion of stars of types G and K: also he identified bands due to hydrocarbons and cyanogen in the heads of Comet Daniel (1907) and Comet Morehouse (1908) and bands due to nitrogen at low pressure in the tails.

In 1927 the observatories of Paris and Meudon were united and Deslandres was appointed the first Director of the combined observatories, a post which he held till his retirement in 1929. He remained always full of original and fertile ideas and encouraged his staff to great activity. Always young in mind and combative by nature he lived a very full life consecrated to astronomical research. His untiring zeal and spirit of invention were linked to a sure and weighty judgment. Elected to the Académie des Sciences in 1902 in succession to Faye he soon became an active member of its various scientific commissions especially for international conferences. Among these may be mentioned the first conference on Solar Research at St Louis in 1904, the first meeting of the International Union for co-operation in Solar Research at Oxford in 1908, and a later meeting at Mt Wilson in 1911. He was present at Brussels in 1919 as a delegate from the Académie des Sciences when the International Research Council and the International Astronomical Union were founded. He attended the meetings of the latter body at Rome (1922), Cambridge (1925), Leiden (1928), Paris (1935) and Stockholm (1938), and was for a time a Vice-President of the Union. He served on many committees such as those on the solar atmosphere, solar radiation, stellar radial velocities, stellar classification, lunar nomenclature and longitudes. In 1908 he became a member of the Bureau des Longitudes and wrote a 'Résumé de Physique Solaire' in the *Annuaire du Bureau des Longitudes* in 1919.

He became President of the Académie des Sciences in 1920 and was the senior member of the Academy at his death. He won the Croix de Guerre, 1914-18, was a Commander of the Légion d'Honneur and was awarded the order of the Crown of Roumania and the Grand Cross of the order of Isabella la Catholique. He was elected an Associate of the Royal Astronomical Society in 1904 and received the Society's Gold Medal in 1913. He received the Catherine W. Bruce Medal of the Astronomical Society of the Pacific and honorary doctorates from the Universities of Leiden and Coïmbra. He was a foreign member of the Academy of the Lincei, Academy of Petrograd, National Academies of the United States and Sweden, and of the Royal Society of Uppsala. He was elected a Foreign Member of the Royal Society in 1921.

He died on 15 January 1948.

F. J. M. Stratton

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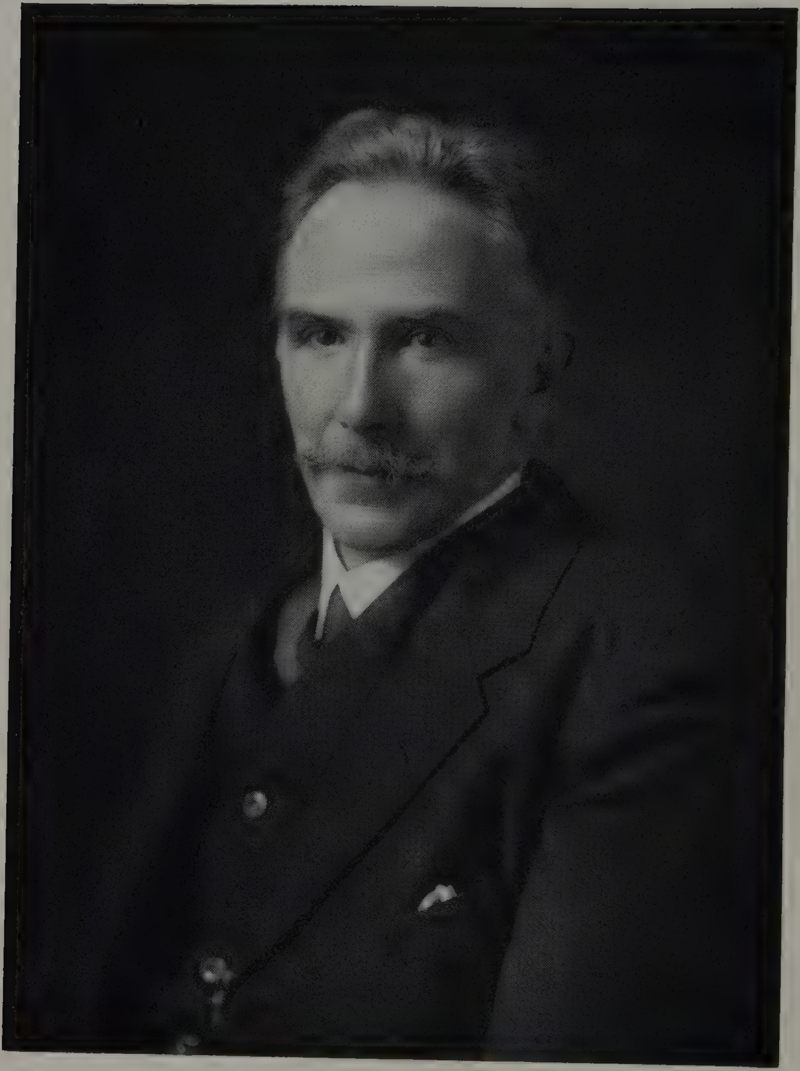

Henry H. Dixon

HENRY HORATIO DIXON

1869-1953

THE first issue of *Notes from Botanical School of Trinity College, Dublin,* opens with an article on 'The Herbarium of Trinity College: a retrospect', which ends as follows with page 14,

'As has already been explained the Cape Flora is for the present kept apart from the rest.

'In 1893, the Board granted the use of another room to the Herbarium, the one in which the British Collection is now placed. This enabled this collection to be consulted during the hours of daylight, without the routine work of the General Herbarium being interfered with, and has in many ways improved the comfort of the place. They further have fitted up the rooms, at one time devoted to the Geological Collection, as a Botanical Laboratory, and appointed Mr Henry Dixon as assistant to the Professor of Botany. In this Laboratory each student is provided with a microscope and all the necessary apparatus and reagents for the investigation of plant structure. During the seven weeks of each term, in addition to the lectures, demonstrations are here given, and the student is encouraged to work out details for himself at any hour of leisure from his other College work. Fresh material is received each day from the Curator of the College Botanical Gardens, F. W. Burbidge, M.A. In their senior year the students are invited to work out some original problem in plant structure or physiology, or to investigate some named family of plants, and prizes are awarded for the best essays on such. Prizes are also given for collections made within a specified time, attention being paid to the care with which the specimens are dried and named, no merit being given for rare species.

'The Botanical Department is still deficient in some modern instruments of research and in many works of descriptive botany, yet it can fairly be stated that the student has at his command all that is really necessary for even an advanced course of botanical study.

'Perhaps, some day, the small addition of a little lean-to glass-house in the College Park may be granted to the department, so that experiments on the growth of seeds and plants, a few relating to plant physiology, and the like, might be conveniently carried on. Such cannot be conducted with certainty in our Botanic Garden Stove or Green Houses without a too serious interference with their proper work, and the distance of the garden from the

College precludes the idea of such a structure as is needed being usefully erected there.

Ed. Perceval Wright, M.D.,

Professor of Botany in the University of Dublin; Keeper of the Herbarium of Trinity College, Dublin. January 30th, 1896.'

The foregoing quotation gives a good idea of the status of Botany in Dublin no further back than when I was a schoolboy and when, in 1894, Henry Dixon was appointed as assistant to the Professor. He had been born in Dublin on 19 May 1869, the youngest of seven brothers. The family included two sisters. The father, George Dixon, owned a soap works; his brother, the Rev. Robert Vickers Dixon, was a Fellow of Trinity College, and Erasmus Smith's Professor of Experimental Philosophy—and later on Archdeacon of Armagh. Seldom do seven brothers have such successful careers, six in their native city, for in Ireland most families have some members abroad. Robert and James managed their father's enterprise, George Yeates was a barrister, William Vickers, Chief Inspector of Technical Schools, and three were university professors, in engineering (Stephen Mitchell), anatomy (Andrew Francis), and botany (Henry). Mr George Dixon died in 1871 while his children were still young, so the burden of upbringing fell on his widow, Rebecca, a daughter of George Yeates of Dublin. She made a home for all and with her daughters—devoted aunts—had a welcome for their friends, and later for their pupils. When one considers this large family, so hospitable, so full of interest in many subjects, it becomes clear that in the quiet old lady with her smile and her lively mind lay the secret of it all.

After attending Rathmines School under Dr Benson, in 1887, Henry Dixon entered Trinity College, the sole College (founded by Queen Elizabeth in 1591) of the University of Dublin. He obtained a classical scholarship in 1890 and took prizes in Italian, his knowledge of which led, later on, to his being the first British botanist to recognize the value of Ricca's work in 1915 on the part played by a hormone in the movements of the petiole of *Mimosa*.

In 1891 he graduated with first-class honours and a large gold medal in natural science, which in Dublin meant botany, geology and zoology. The last had for long been under the care of Professor H. W. Mackintosh, a wonderful lecturer, who looked down his microscope and saw the marvellous works of his Creator. Geology was taught by the Rev. Samuel Haughton, M.D., D.C.L., F.R.S., a veritable polymath, whose name figured in *Proceedings of the Royal Irish Academy* as author and office holder from 1846 to 1897. The Chair of Botany was filled by Edward Perceval Wright as previously mentioned. He was Sir Almroth's uncle and as a traveller had enriched the lovely Botanic Garden; the Herbarium, already large, owes much to his labours as a keen systematist.

The era in which Dixon entered the University of Dublin may indeed be regarded as one of its most vigorous. In addition to those mentioned there were the physicists George Francis Fitzgerald and John Joly. In chemistry we find Emerson Reynolds, with Emil Alphonse Werner as a keen youngster.

Edward Dowden adorned the Chair of English Literature—and his brother was Bishop of Edinburgh. There was Lecky the historian, Tyrrell the Greek scholar, the versatile Mahaffy and the young Charles Jasper Joly, mathematician and astronomer, with the older mathematicians Williamson and Tarleton also Salmon, mathematician, theologian, Provost. Thus Henry Dixon graduated in an atmosphere of intense intellectual activity as yet unshocked by war. After graduation he studied at Bonn under the celebrated Professor Eduard Strasburger. In 1894 he was appointed assistant to Professor Wright whom he succeeded when Wright retired in the summer of 1904. In 1906 Dixon became Director of the Botanic Garden, and after Wright's death in 1910, Keeper of the Herbarium also. In 1924 he was appointed Regius Professor of Botany in the University of Glasgow, but when informed of the new age limit for retirement he declined the post.

No account of Dixon's moulding would be understandable without mention of his friend John Joly, his senior by some fourteen years. The families lived in the same road, and later moved to adjacent suburban homes. Joly, the brilliant physicist, found his young neighbour a congenial companion in the Alps and on his yachting cruises. The benefit to a botanist of being able to discuss physiological problems with a competent physicist, was, of course, great. Thus the names of Joly and Dixon were linked for ever as the originators of the cohesion theory of the ascent of sap. In many university and other activities their names were also associated, and an ideal friendship of some fifty years was terminated in 1933 by Joly's death. To his friend he left his house, his family heirlooms, his pictures, his books, and it was upon Dixon that the Royal Society of London called to write an outline of Joly's life.

In 1907 Professor Dixon married Dorothea Mary, daughter of the late Sir John H. Franks, C.B. Miss Franks was a graduate of the Royal University of Ireland, and had entered Trinity College, Dublin, as a medical student when under Provost Traill ladies were admitted to degrees, a right then denied by other older universities. The hospitable traditions of the family were maintained in his married home, and many students and members of the University staff have the most happy thoughts of their kindness, none more so than the writer, whose privilege it was to experience a Dixon welcome over the long period of fifty years—and how fresh are these memories! Both parents had great joy in the successes of their sons, the eldest Joly being a nerve specialist, a Member of the Royal College of Physicians in practice in Glasgow; his slightly younger brother, Kendal Cartwright, lectures in pathology at Cambridge and is a Fellow of Kings, to which dignity the youngest brother has also been elected as a biochemist.

Through the generosity of Viscount Iveagh* a new School of Botany was erected in 1907. It bears throughout marks of the Professor's forethought. No building ever had less space wasted in passages, etc., or rendered useless by inadequate lighting. Its plan and elevation are figured in the British Association *Handbook* to the Dublin District (1908). Much time was spent in adding to the already good cabinets of plant sections or mounted micro-flora, also in preparing hundreds of lantern slides from photographs of the slides or of text-book figures. The courses in the School were also illustrated by specimens received from its own Botanic Garden, a little over a mile away. This, which replaced an earlier garden elsewhere, had been opened in 1806. It was walled in early in 1807, and in the spring of 1808 the planting of trees, shrubs, and herbaceous plants commenced.

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* Later the first Earl of Iveagh.

In his article in the British Association *Handbook*, Dixon wrote, 'The School of Botany is also fortunate in possessing a Botanic Garden of 8¼ acres,* situated at Ball's Bridge, and founded in 1806. The inner garden contains a collection of the principal natural orders of hardy plants for teaching purposes; while in a series of seven glasshouses are grown tender and tropical exotics; there are also several ponds for the cultivation of water and marsh plants, and suitable conditions are provided for the cultivation of trees, herbaceous, alpine, and bulbous plants.' Thus we see that the Garden was already old and well stocked when Dixon took charge, yet he improved it and introduced more than seven thousand species, including Arthur Kerr's notable collection of Siamese orchids which Sidney Wild, the Cambridge-trained head gardener, tended so sedulously. The re-painting of the labels was whole-time work for one man, but Dixon started to replace them with black and white glazed stoneware supplied by a German firm. When his assistant, our work in Michaelmas term included the listing of the labels which were required and the drawing up of the seed-lists for exchange with other gardens, taking care to bring all into conformity with the *Index Kewensis*. We were not, however, limited to our own gardens for specimens. Sir Frederick Moore, Director of the Royal Botanic Gardens at Glasnevin was always most helpful in supplying our needs, especially those from their fine collections of tree-ferns and other tropical plants. Dublin is also fortunate in having within easy reach both clean rocky coasts and mountains with woods, streams and bogs, acid soil, as well as limestone, sand dunes and Palaeozoic areas at Howth and Bray Head. The latter provide habitats very well suited to inland, maritime and marine lichens.

In the heavy task of organizing and equipping the new School, the Professor was nobly aided by his assistant Louis Bouvier Smyth, who graduated with first place in the Natural Science Moderatorship Examination in 1906, and after Joly's death late in 1933, succeeded him in the Chair of Geology. Nobody had more cause to value Smyth's work than had the writer, for following Arnold Henry—a medical stop-gap for a year—I came across from Chemistry, full time, in 1911 to conduct a course already well provided with teaching requirements. Furthermore, very shortly after his appointment, the Professor took on a red-headed boy, Joe, and Mr Joseph Murray was truly a pillar of strength to him till after his retirement. When in 1937 Dixon delivered the Croonian Lecture it seemed natural that Joe should be there also. Mr Murray died rather suddenly in January 1954. Another very likeable figure seen in the School was Mr Polly, the head groundsman of the College. But the great event daily was the arrival of Professor Joly for a tea and toast lunch with our Chief. The assistant, demonstrators and students were often invited—and life sparkled.

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* I am indebted to E. J. Bourke Esq., City Surveyor and Waterworks Engineer for kindly ascertaining for me the exact area.

The elementary lectures were always delivered by the Professor; each was followed by a practical class in which his assistant and demonstrators taught under his supervision. The class also visited the Botanic Garden. This course was published in 1922 as *Practical plant biology* and a second edition appeared in 1943. Starting with the use of the microscope, it deals first with the structure and physiology of unicellular plants and bacteria, and simple cultures are made. The medical student is thus, at the start of his career, brought to understand the meaning of infection and sterilization. Special attention is paid to the real appreciation of the sizes of objects seen under the microscope, measurements being made in three dimensions. To facilitate this Dixon introduced a 'ghost micrometer', a strip of glass held on a stand in front of the mirror of the microscope. This had a photograph of a grid of squares, and by manipulation of the condenser the grid could be seen with the objects on the stage. Calibration of the grid under the various objectives was simply carried out with a micrometer eyepiece. Before hitting upon this simple device he used a suspension of yeast cells, each about 10 microns in diameter.

Types of algae and fungi are then considered, followed by the liverworts, ferns, gymnosperms and angiosperms. The whole course is constructed to illustrate evolution, and the final lectures deal with this, nuclear division and theories of heredity. Thus one does not wander from the green leaf, root and flower back to the unicellular organisms, but progresses mentally in the same direction as was followed in plant evolution and is followed in its life history by every plant. This course is recognized by past students as constituting a landmark in their education, and its creation must be reckoned as Dixon's greatest success as a teacher. Many years after he had attended it a distinguished surgeon told me that it was the first thing in his education that had really made him think. For the practical classes, which might run to 150 in peace time, about a dozen demonstrators were taken on.

The elementary course, in rhyme, even found a place in the College magazine, *T.C.D.* The Professor liked the version and asked for a copy.

One stanza may perhaps be quoted:

> 'Sphaerella and the giddy fast-gyrating Volvox soon
> Shall come again and eddy in the light of April's moon
> When the ovules of sweet Lilium are mitotically engaged
> And the carpels of Ranunculus are in golden yellow caged.'

Those taking the course in Natural Science for an honours degree (Moderatorship) specialized in the last half-year of the four years' course in one subject out of the three. It was held, and the writer still holds most emphatically, that an early specialization in any one science is most deplorable. The very limited numbers taking the honours course ensured that under Dixon each student received that personal contact which can prove so stimulating. Old students who returned had the pleasure of realizing that the courses were undergoing a ceaseless revision and were being illustrated by the Professor's new lecture experiments or beautiful cytological preparations.

Soon after Wright's death Dixon succeeded in having an addition made to the east end of the school, so that by 1912 the Herbarium was housed better than ever before, in a lofty well-lighted room, together with many floristic works. The labour of moving, sorting and treating hundreds of bundles to destroy insects was immense, and was carried out with but little assistance. Its library included the books presented by Dr E. P. Wright. Among its possessions may be noted a British collection, rich in species and varieties, including the gatherings of Mackay, Greville, Balfour, Henslow, A. G. More, W. H. Harvey and others; and a collection of algae, with many type-specimens, the work of Harvey, Agardh and Mueller.

One thinks of Dixon as a physiologist, but in his earlier years he made important contributions to cytology; these include work on the chromosomes and the first mitosis of the spore-mother cells of *Lilium* (which described for the first time the real behaviour of the chromosomes in meiosis), as well as work on the mitosis of the nuclei of the endosperm of *Fritillaria imperialis*. He also studied the resistance of seeds to high and low temperatures and to poisons. His experience when studying the temperature of the underground organs of plants led to his thermoelectric method of cryoscopy, used in so much of his later work on osmotic pressure. As a memory of his love of sailing we have his papers with Joly on coccospheres, coccoliths and the marine phytoplankton of the east coat of Ireland.

Outside the University, Dixon's activities were largely connected with the Royal Dublin Society, which fosters scientific research, agriculture, music, distributes radium emanation, and runs the famous Dublin Horse Show and Agricultural Shows. In this he served on various committees and on the council. He was for many years a Vice-President and was President from 1944 to 1947, the normal term of three years. He was also on the committee of the Imperial Bureau of Mycology, London, a Visitor of the Glasnevin Botanic Gardens, a Trustee of the National Library of Ireland, and a Commissioner of Irish Lights. The latter involved a yearly inspection of lighthouses and lightships, a service still maintained by the British Government. To be hauled up a lighthouse and lowered again into a small boat in the grip of Atlantic rollers is no nominal undertaking.

In 1908 Dixon was elected to the Fellowship of the Royal Society of London, and in 1917 he received the Boyle Medal of the Royal Dublin Society. In 1922 he was President of the Botanical Section at the British Association Meeting in Hull. The photograph reproduced in this article shows him as he then was.

In 1927 Dixon went as visiting professor to the University of California, a result of the exchanges initiated by the British Educational Mission which had visited the States a few years earlier. The beauty of the country, the vast scale of the University, the keen scientific discussions, and the warmth of the personal welcome made a deep impression upon both the Professor and his wife. The visit was marked in their family history by the christening of their youngest son as Henry Berkeley, born in 1928. In 1937 Dixon was elected a Corresponding Member of the American Society of Plant Physiologists, an honour which brought him pleasant memories of his friends. In 1948 he received their Charles Reid Barnes Life Membership award.

In 1949 both H. H. Dixon and E. A. Werner were elected honorary fellows of Trinity College, Dublin; Dixon then 80 and very fit was 'dined on' in June. Poor Werner I found very cheerful at 85 but confined to bed with a sudden attack of bronchitis. To him also I am deeply indebted for much inspiration and kindness.

At the end of the Michaelmas term, 1949 Dixon resigned his Chair, but for some time longer he retained his post as Director of the Botanic Garden. It was with great pleasure that my wife and I met the Professor, his wife and our own son, recently one of the Professor's students, when their ship reached Göteborg in early July 1950. But after visiting the beautiful Trädgården the Dixons went on to Oslo that day, and we met later at the Seventh International Botanical Congress, Stockholm, of which he had been elected an Honorary President. By this time the doyen of British botany, not very well before leaving Dublin, had recaptured an amazing buoyancy of mind and body. We had a delightful time at various functions and excursions, and the Professor contributed two short papers in the section of plant physiology. Sir John and Lady Russell were at the Congress also, and it was pleasant to see how cordially both men were greeted everywhere.

The Belfast Meeting of the British Association, 1952, was also attended by Professor Dixon. He had been there in 1902.

We see thus a great teacher and organizer, who had built up a fine school of Botany and had done much research in spite of very limited means, very limited because at the time of the 1914-1918 war Trinity had backed the horse that won in Europe but lost in Ireland. The post-war Royal Commission had recommended large initial grants to Oxford, Cambridge and Dublin, to be followed by annual grants. Then came the Lloyd George surrender and Trinity got nothing, till about twenty-five years later Mr De Valera's government gave £30000, a great help.

Some thousands of our graduates and students served in the 1914-1918 war and over 460 fell—as volunteers, for conscription was not applied in Ireland. Dixon felt very keenly his inability to be of more active use, but hearing from friends in France of the dire need of sandbags, he and his wife organized working parties; they made and sent out thousands of the bags so sorely wanted. Also when shell parts began to be made at the Inchicore Works of the Great Southern Railway, he rode out daily after his morning lectures and worked at gauging the parts till a team of girls could be trained in. Later on when I appealed to him for help in the identification of aircraft timbers he examined my specimens, sent from Egypt, and got many others. Such was the origin of his paper on the identification of different kinds of mahogany, published in 1918 by the Royal Dublin Society with many costly plates, an expense which became a profit as the entire issue was bought up, mainly by the timber trade. When we met again the Professor told me that among all the specimens there had been no new structure or combination of special botanical interest.

It was typical of Dixon that he worked with the simplest apparatus, each step having been thought out carefully. These methods he applied to the needs of living, such as that of boiling a kettle with damp gorse in the wilds of Kerry. Thick leather gloves, a wire cutter, and an iron rod ending in a hook gave the means of applying small pieces of combustible matter immediately below the centre of the kettle. Enough boiling water to make tea for six or seven people was thus produced in ten minutes in a field soaked in a Kerry mist!

In the list of references there is one to a letter, in *Nature*, on mercury bubbles. This he never mentioned to me and for the first time I read it last week. These surprising bubbles he observed on boiling water over a layer of mercury. The bubbles consist of 150 to 200 mg mercury and may last 15 to 30 seconds. The diameter was observed to be just under 20 mm—and one bubble of this size lasted 75·6 sec. From these observations the mean thickness of the film was found to be 10 microns, thinner in places as a thickening developed on the under surface. So much was thus found out so simply—read it and see! But his first publication concerned the walking of arthropods and was dated from the Physical Laboratory, where he took instantaneous photographs of small animals.

Pressure of other work prevented the following up of some of his early papers. He showed how to grow seedlings in sterile culture. He suggested and figured the differential manometer for the study of respiration rates. This was published in *The Notes*. His study of the action of radium on seeds and bacteria, carried out with J. T. Wigham, furnished a theme for many other workers.

But the main line of Dixon's work for many years arose out of his collaboration with Dr John Joly when in 1893 they published the tension theory of the ascent of sap in trees. In this work a problem of long standing was attacked, and a rational explanation found as to how the sap is raised in high trees. The theory, now generally accepted, led to physiological and physical experiments of a high order. It was supported also by the anatomical study of the conducting tracts. Among the researches carried out by Dixon in this connexion were investigations on transpiration into a water-saturated atmosphere; on transpiration into various vapours and gases; on transpiration in dead branches; on the tensile strength of water; on the tensile strength of sap, and on the strength of cell walls; also on the resistance experienced by the transpiration stream. In these researches he further established the tension theory, replied to criticisms on it, and showed that the cells of the leaf have often an active part in applying the tension to the sap; also that this tension is transmitted downwards through the tree, and raises the sap in the stem without the intervention of any forces due to the living cells of the stem. Using his thermo-electric method of cryoscopy with sap pressed from leaf cells he was able to determine their osmotic pressure and to prove that this was adequate even in the case of very tall trees. He had previously partly solved this problem by a direct method, abandoned on account of its danger. Watching the reflexion of the experimental vessel in a mirror he observed that the walls had begun to bulge—and then!

To join in the work on the cryoscopy and composition of sap was my good fortune. In spite of the evidence to the contrary, afforded by an examination of the pressed sap under the microscope, it was shown by Marie and Gatin that the usual method of extraction by direct pressure, which we and others had used, did not in fact give the true concentration. Immersion of the tissues in liquid air suggested itself to the Professor as the way out. The cells are rendered permeable so that even zymase can be obtained from yeast.

But for the study of the transpiration stream it was desired to get the liquid out of the vessels and tracheids, free from contamination by the contents of the living cells of the medullary rays or phloem. What about spinning pieces of a woody stem in the Leune centrifuge? This query, posed by the Professor, started us on an examination of the sugars in the sap, of its osmotic pressure and electrical conductivity. The carbohydrates stored in cells of the parenchyma and medullary rays became mobilized as sugars in the spring and, passing into the transpiration stream, these were carried up to supply the needs of the young branches and leaves. But as shown by others later, the downward supplies from the leaves in active photosynthesis pass in the phloem—hard though this is to explain in terms of the flow of liquid.

This very happy collaboration was ended by the war. The drawback of working with our Chief was just this—nowhere else ever seemed as good. But at least like the cat that lays its mouse on the doormat for its mistress to see, one could have the fun of bringing back—or posting back—results for criticism or approbation, and to several of us the Professor extended the courtesy of a place in *The Notes* for reprints of papers published by the Royal Dublin Society. *The Notes*, begun in 1896 were interrupted, like much else, in 1938, the last paper being Dixon's 'Subaqueous transpiration', a concise and conclusive reply to criticism of his earlier work. He showed, taking additional precautions to ensure that there was no water deficit in the leaves and stems, that the leaf cells do in fact abstract water from the tracheae and secrete it on the outer cell surfaces.

Harking back though to earlier days, in 1909 Dixon was asked to contribute to the international journal *Progressus Rei Botanicae*. The resulting communication afforded a standard account of the theory of the ascent of sap and of the experimental evidence which had been accumulated around it.

This publication was, however, brought up to date and extended in *Transpiration and the ascent of sap in plants* (1914). The work is a model of exact scientific method applied to the complex conditions prevailing in the plant. When it is remembered that the greater part of the book is a record of methods devised by the author himself and, although in some places carried out in collaboration with his pupils, yet always guided by his own scientific insight and learning, it does not seem too much to hope that it will remain an enduring monument of the author's principal contribution to botanical science. Save for the first six words, the foregoing paragraph is almost an exact quotation from an unsigned report issued by the Royal Dublin Society in 1917, when its Boyle Medal was handed to Professor Dixon by the President, The Right Hon. Lord Rathdonnell, H.M.L. From internal evidence I feel sure that the author of this report was Professor John Joly.

Just too late for the 1914 book was an extension of some earlier work, in which the Professor and Miss Marshall showed that a quantitative examination of the areas of the wood of trees afforded support to the functions assigned to such tissue. Then followed work on the primary sugar of photosynthesis, with T. G. Mason, who after the war launched forth into a notable series of papers on the movement of sap constituents in various tropical plants. 'The transport of organic substances in plants' was the subject of Dixon's Presidential Address to the Botanical Section of the 1922 British Association in Hull, as also of his paper at the Toronto Meeting in 1924. His three lectures before the University of London, published as *The transpiration stream* (1924) brought the subject up to date, including his work with Ball, who took my place in 1920. Sir Nigel G. Ball, Sc.D., has kindly supplied the following outline of their joint work and of cognate work by Dixon and others.

'It was my privilege to be associated with some of Professor Dixon's researches between 1919 and 1924. First of all, at his suggestion, a sensitive thermocouple such as he had employed in some of his earlier work was used to measure the heat produced during the inversion of sucrose. By this means the gradual rise in temperature of the experimental flask above that of a control, amounting in all to about 0·4° C, was followed easily and accurately.

'Later, in continuation of earlier work by Dixon and Poole on "Photosynthesis and the electronic theory" we examined the effect of chlorophyll on the sensitization of photographic film to red light at the temperature of liquid air. Using collodion plates a positive result was obtained. This suggested that the formation of the latent image in red light was due to displacement of electrons in the chlorophyll molecule, since chemical action was out of the question at the low temperature. Progress in recent years towards the elucidation of the mechanism of photosynthesis has been so marked that early experiments such as these are now of little interest, but the fact that even before 1920 Dixon had begun to think of photosynthesis in terms of electronic displacement is worthy of record.

'About the summer of 1921, Dixon became interested in the problem of the transport of organic substances downwards in plants. Calculations based on the amount of carbohydrate passing into a potato tuber during the growing season and on the cross section of the phloem seemed to indicate that this tissue was quite inadequate as a conduit. The only other available conducting tissue was the xylem, and reasons for thinking that this was in fact responsible for transporting organic substances were given in a joint letter to *Nature* in February 1922. These arguments were followed up and extended by Dixon in his Presidential Address to Section K of the British Association the same year. Later, in the hope of obtaining further evidence as to the tissues responsible for transport, we commenced a study of the transmitting regions of the cotyledons of certain seedlings. Large quantities of organic substances are translocated through these regions during germination, although the amount of water passing through them is very small. Dixon himself undertook the major task of investigating the seedlings of *Lodoicea* and *Phoenix*, while I studied that of *Vicia*. The presence of large amounts of xylem in regions where it did not appear to be required for water transport seemed to support the idea that it might be responsible for conducting carbohydrates.

'As early as 1896 Dixon had estimated the osmotic pressure of leaf cells by exposing branches to compressed air and had found that by this means it was possible to cause a backward flow in the xylem. At his suggestion we repeated the experiment in 1923 to see whether or not the liquid forced out of the wood contained sugars. On exposing branches to 20 atm., liquid emerged from the cut end of the stem. This liquid was found to be free from sugar. Although this experiment gave no support to the hypothesis that sugars moved downwards in the wood under normal conditions, the arguments in favour of this view still seemed very strong. They were summarized soon afterwards by Dixon in one of the three lectures given before the University of London in January 1924.

'The chief value of Dixon's earlier contributions to the problem of the translocation of organic substances in plants is that they served to clarify the issues and to draw the attention of others to the need for further research. Prominent among these was his former student, T. G. Mason, who, in collaboration with E. J. Maskell and later with E. Phillis, carried out a long series of investigations on the movement of organic substances in the cotton plant. These experiments showed that the phloem was, in fact, responsible for the transport. Dixon himself soon accepted this view, which has since been abundantly confirmed, although the way in which the movement takes place has still not been satisfactorily explained. In his paper on 'bast-sap', published in 1933, he showed that liquids could be forced under pressure through the sieve tubes, and produced evidence for the existence of a gradient of osmotic pressure in the bast-system. In 1936 he published the results of ingenious experiments on the convection of heat in stems. These gave an indication that a downward movement in the phloem could occur at the same time as an upward movement in the wood. Finally, in his Croonian Lecture to the Royal Society in 1937, he devoted some attention to the transport of organic substances and reconsidered the problem in accordance with what was then known.'

In an old letter, preserved in the gift of his 1914 book, I have his account of the work, referred to by Ball, and published in 1936. 'I believe I have got very beautiful evidence of downward mass movement in the bast.' Then follow two diagrams, the plan and profile of his heating element with two thermo-junctions inserted in a stem, and the galvanometer deflexions given by the junctions when the heat was switched on and off several times within 200 minutes. Thus simply by distinguishing between heat transferred by convexion and by conduction he was able to follow the flow down and up in phloem and xylem respectively.

With this letter lay an older one, of March 1924, and in it 'I have gone over C——'s work again and I don't see why blocking won't explain all his results. Injury to the living cells must act in two ways, (1) by introducing pathological substances into the tracheae and, (2) by stopping the introduction of digestive enzymes the function of which is to keep the tracheae clear. It may be significant that the companion-cells, the medullary ray cells and wood parenchyma cells generally have the granular protoplasm and large chromatin-rich nuclei characteristic of glandular cells.' All through Dixon's work one finds this close linking of form and function.

When Ball left for Ceylon in June 1924, Dixon had already turned his attention to the study of variations in the permeability of leaf cells, with which transport through the phloem was closely associated. This was continued with his new assistant T. A. Bennet-Clark, who remained till December 1930. The very fruitful collaboration resulted in six important papers, the last appearing in 1932. Such arduous experimental work did not, however, chain to earth Dixon's soaring probes into evolution, a topic never very far from his thoughts. A letter to *Nature* in 1929 suggested that cosmic radiation, by its random influence on the genes, might influence evolution and 'The control of differentiation' was his topic at the Amsterdam Congress in 1935. Of the work and period during which they were associated Professor T. A. Bennet-Clark, F.R.S., has kindly furnished the following account.

'Dixon's interest in the problem of transport of organic materials developed around the time of his Presidency of Section K of the British Association.

'He was particularly impressed (following his own work on rates of transport) by Ricca's demonstration of downward transmission of the hormone stimulating the pulvinus of *Mimosa*. This was shown by Ricca to move in the xylem almost certainly.

'It suggested to Dixon a mechanism by which the xylem might carry both upward and downward currents of water or solutions: the idea was that a local loss of semipermeability of leaf cells would cause an emptying out of those cells due to their former high hydrostatic pressure and the tension in the xylem columns.

'This inspired a series of studies of factors influencing permeability of leaf cells. The method developed was measurement of electrical conductivity which, broadly, gave an estimate of permeability to ions.

'The effects of a variety of stimulation treatments were studied (with his assistant at the time, T.A.B-C.) and, largely on a basis of analogies with studies of nerve and muscle, the effects of electrical stimulation were specially examined. Such stimuli are effective in evoking the *Mimosa* response and characteristically cause increases in permeability of leaf cells. It seemed likely from parallel studies of effects of electric fields on water-in-oil emulsions that the mode of action of this electrical stimulation was a temporary breakdown of structure of plasmatic membranes.

'It is, of course, the case that while these studies were in progress the work on transport of organic materials in the cotton plant were being undertaken in Trinidad by one of Dixon's former students in collaboration with Maskell (now Professor Maskell). It became increasingly clear to Dixon that it would be necessary to modify his first view that phloem transport was impossible because of the high rate of transport in tissue which seemed anatomically unsuitable. The ambiguities in the available evidence were certainly subjected to a very searching scrutiny before Dixon could bring himself to think of phloem as a major channel of transport—but to this day the question of mechanism of transport remains a complete mystery surrounded by such unhelpful expressions as 'activated diffusion'.

'At about this time Dr Brauner (now Professor at Istanbul) spent a year in T.C.D. with a Rockefeller Fellowship and worked also on permeability problems demonstrating polar movement of water through seed coats and developing a hypothesis of its mechanism as an electrosmotic phenomenon.

'Other research in the progress in the School of Botany at this time (1924-30) consisted of studies of the metabolism of succulent plants begun in Cambridge (by his assistant T.A.B-C.).'

Professor Dixon's seventieth birthday was celebrated on 19 May 1939 by a gathering in the Fellows' Common Room of Trinity, when the Provost, W. E. Thrift, presented him with a congratulatory address in the presence of the leading scientific men of Dublin, as well as of many members of the University. The address was signed by his students past and present, by the staff of the University, and by leading botanists and representatives of learned societies, research institutions and commissions in many lands. The signatories numbered about 560. Absent in 1939 I had the privilege of attending the family party in celebration of his eightieth birthday. His sense of humour and his keen enjoyment of life remained unabated. He was, I think, the most uniformly serene and happy man I have ever known. Never did I think I should break off the reading of Joly's obituary, as written by Dixon, with a quiet chuckle, but I did—as he recounted one of the impish pranks of Joly's boyhood. An account of the above-mentioned presentation appeared in *The Irish Times*, and on the reverse were articles on the Stewart Institute for Imbecile Children and the neglect of history in Irish schools. I am sure Dixon laughed at 'And who was Cleopatra?' 'She rode through the streets of

Coventry on a white horse.' 'And where is Coventry?' 'In Greece.' 'Do you learn history at school?' 'No, only Irish history.' 'Well, who founded the city of Dublin?' 'Brian Boru.' Personally, I do not believe it. But stranger things have happened—so it concluded.

Ball has already referred to the work, cognate to that while he was assistant, which culminated in the Croonian Lecture. Then came, in 1939, with John S. Barlee an ingenious experiment whereby the tension in the sap could be demonstrated by leaf movement—the method was explained also at the Stockholm Congress.

As previously mentioned Dixon returned again, in 1938, to the mechanism of transpiration and did so yet once more in 1940 with Barlee. Taking the necessary precautions they showed that saturation of the surrounding water with air and the evolution of oxygen in photosynthesis favoured subaqueous transpiration; also that replacement of oxygen with nitrogen depresses and ultimately inhibits the upward movement of water. Anaesthetization reduces or stops it. Here we find the last experimental work he published.

This time war brought larger numbers to Dixon's classes. For though the blood of their fathers impelled many T.C.D. students to fare forth, there were yet male persons who found the neutral South of Ireland an equivalent to the crossing of Jordan into the promised land and persuaded themselves that residence in Dublin was unlikely to involve any premature citizenship of Jerusalem the Golden. I have been credibly informed that the five bombs which somehow did fall were wonderfully efficacious in the promotion of piety.

Heavy teaching duties, lack of many things—of gas in particular—and increasing years brought research to a standstill. But, probing the past, Dixon's restless mind suddenly perceived a meaning for the arrangement of the dividing nuclei in the endosperm of *Fritillaria imperialis*, in the slides long prepared and by then almost objects of veneration. He suggested a mitotic hormone and this, with eighteen beautiful plate figures was, in 1946, his last contribution to the *Scientific Proceedings of the Royal Dublin Society*, though his Presidency of the Society extended till December 1947.

The mitotic hormone was also his second subject at Stockholm. I recall that Professor H. W. Mackintosh, in his lectures on malaria, would pull his puzzled class together, with 'And you will ask me where this is—and I will tell you that it is in the stomach of a mosquito—and you will ask me why—and I will tell you that I do not know.'

Dixon too asked why—but the why of nuclear division is unlikely as yet to be answered, though the suggestion of its initiation by a diffusing substance may very well be the child's first step to knowledge—by the joint-author of the *Tensile theory of the ascent of sap*.

Shortly before leaving 'The School' he wrote 'Few can have experienced such continual kindness and help from assistants and pupils—more than kindness—self-sacrifice and affection.' Have you seen how dim bright stars look when seen at night in calm water? The reflexion was what reached our Chief.

After his retirement Dixon spent much time in his bungalow at Dooks, Co. Kerry. This when enlarged held the two younger families also, with four grandchildren. In Dublin tennis had been his early recreation, and indoors his pianola, played with great expression and musical appreciation—Wagner he loved especially. But at Dooks there was bathing, golf and sketching. He and Mrs Dixon, also the artist friends who loved to visit them, found great peace and joy in the wonderful panorama of their wide valley with its mountain ranges north and south, its river—and the Atlantic tongue to the west. Such loveliness has Kerry, with its smoke-free clearness passing through every intensity of blue haze to the white obscurity of cloud-capped mountain and sea fog—they painted all.

His retirement was not entirely spent in Dooks however—there was Norway and Sweden in 1950, and early in 1952 Italy, with Florence and Venice. But from Kerry—'You asked if Dorothy made many sketches. She only did two or three. . . One picture of Coomnasagharn lake and one of the north side of Dingle Bay from Rossbehy seem to me very lovely.' It was to this lovely lake, which comes in sight so surprisingly in its mountain bowl, that Mrs Dixon drove us the last day I spent with them in May 1952, just after his 83rd birthday. The next year his powers slowly declined and he died in his Dublin home on 20 December. From Oliver Gogarty's *Non Dolet* I quote:

'Our friends go with us as we go
Down the long path where Beauty wends,
Where all we love foregathers, so
Why should we fear to join our friends?
. . . .
Nor, after Caesar, skulk in Rome.'

W. R. G. Atkins

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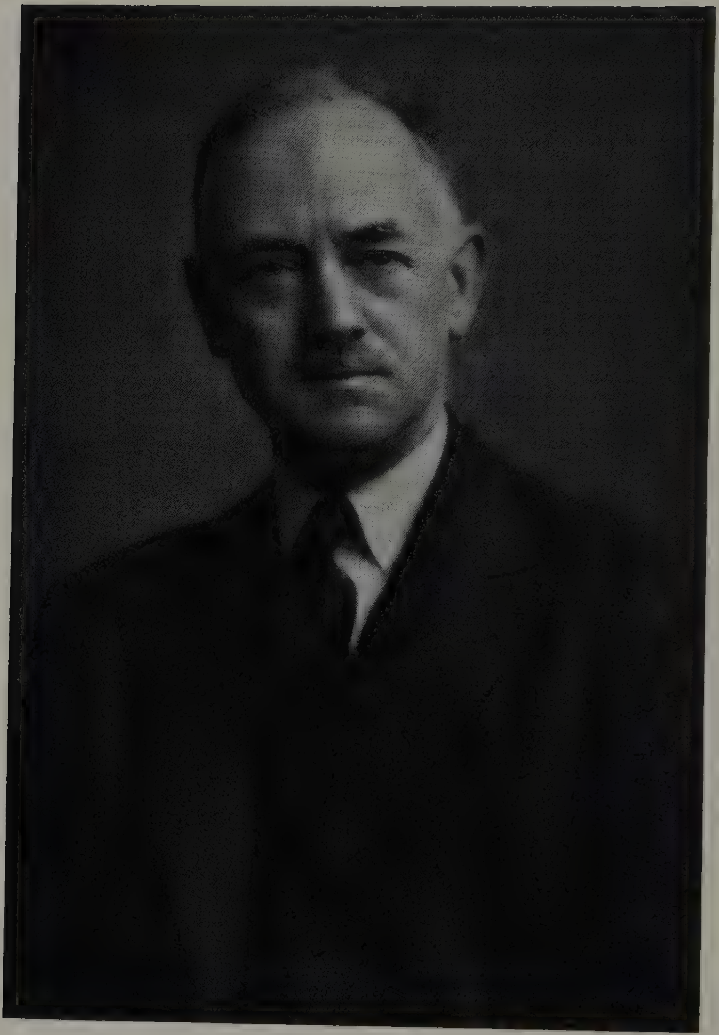

J. C. Drummond

JACK CECIL DRUMMOND

1891-1952

Early Years: 1891-1914

Jack Cecil Drummond, the only child of Major John Drummond of the Royal Artillery and Mrs Drummond, was born at Leicester on 12 January 1891. His school education was received at the Roan School, Greenwich, and at Strand School, London. While he was very young his parents died in India, and he went to live with an elderly uncle and aunt, Captain and Mrs Spinks, Mrs Spinks being the sister of Jack's father. Captain and Mrs Spinks, having no children of their own, brought up young Jack as their own child, but their home seems to have been rather bleak and run on rigid lines, Mrs Spinks being a strict nonconformist and insisting on Jack's accompanying her to chapel until he was quite a young man. Captain Spinks was a Crimean veteran and a keen gardener, and it is probable that from him Jack first acquired an interest in and love for flowers. Certainly it is recorded that from an early age he was absorbingly interested in the countryside, especially in wild flowers, birds and aquatic life. His interest in birds was outstanding until the end of his life.

A talent for drawing showed itself during his boyhood days, and from this emerged an interest in photography which was probably the beginning of a growing attraction to chemistry. Chemistry was a subject in which he excelled at school.

He appears not to have been an especially outstanding schoolboy either with respect to lessons or to sports. He was not particularly robust and showed no great athletic powers although he played cricket with zest. The stimulus of the need to pass the London Matriculation Examination spurred him on to appropriate efforts, and in 1909 he entered East London College (now Queen Mary College) from which three years later he graduated in chemistry with a first-class honours degree of the University of London. At East London College he met Mabel Straw, a fellow student whom he later married and who in time became known to many biochemists from Great Britain and from abroad as a gracious and kindly hostess.

With his military background it was natural that he should early take an interest in Army affairs, and he was in the Special Reserve of Officers, holding a commission in the East Surrey Regiment for a year or so before the outbreak of the 1914 war. In spite of his preparedness his heart was found to be weak in some way, and he reluctantly had to resign his commission.

For a few months after graduation in 1912 Drummond did some research work in organic chemistry under Professor J. T. Hewitt, but shortly came an invitation which determined his whole life's work. Drummond's choice of biochemistry as a career came about by accident and not by design. In 1912 Dr O. Rosenheim was carrying out research at King's College, London, in the Department of Physiology, of which Professor W. D. Halliburton was the Head. At that time Rosenheim was directing among other researches the investigations of Dr W. Bain, a well-to-do Harrogate physician who worked intermittently in the Department of Physiology. Bain was investigating the reported occurrence of pressor bases in normal urine, and in the course of this research it became necessary for him to work up several hundred litres of material, an undertaking for which he required assistance from a chemist. Rosenheim asked his old friend, Hewitt, to recommend one of his advanced students for this purpose, and Hewitt sent Drummond along. Bain then engaged Drummond as his assistant with a modest salary, and after much tedious work Drummond succeeded in isolating about 80 mg of crude *iso*-amylamine oxalate from 400 litres of urine. The purified substance was identified by melting point and microanalysis, a novelty in those days, since Pregl's micro-methods had only just been developed. In the publication (by Bain alone—*Quart. J. exp. Physiol.*, 1914, **8**, 229), Bain acknowledged his indebtedness for chemical assistance to Mr J. C. Drummond, B.Sc.

During this period Rosenheim proposed to Drummond that they should collaborate in utilizing the insolubility of benzidine sulphate to work out a volumetric method for the estimation of the so-called ethereal sulphates in urine. This work was finished in 1913 and published jointly by Rosenheim and Drummond in the following year (1).* Subsequently Drummond himself developed a micro-modification of this method for the volumetric estimation of total sulphur and sulphates in small quantities of urine and this formed the subject of his first independent publication (3).

During his time at King's College Drummond's interest in biochemistry was enhanced by discussions with other workers in the laboratory and by contact with the medical students who came to the classes in physiology; Drummond himself attended these and many other biological and medical classes and ultimately became unusually well-informed in such subjects as histology and morbid anatomy. His biological outlook and his knowledge of physiology was widened by his friendship with one of the most brilliant of the medical students, who unfortunately was killed early in the 1914 war. Professor Halliburton was greatly impressed by Drummond's general ability, and when the grant for assistance to Dr Bain came to an end, was instrumental in procuring an appointment for him as an assistant at the Cancer Hospital Research Institute, Fulham Road, London, S.W.3, in March 1914. However, between his appointment at King's College and that at the Cancer Hospital Research Institute, Drummond became for a few months a junior member of the staff of the Government Chemist, a time which he spent mostly on analyses of steel, although even here, according to his own account, he was given one biochemical problem of a somewhat unorthodox type.

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* Numbers in brackets refer to the bibliography at the end of this notice.

Cancer Hospital: 1914-1919

At the time when Drummond began his career in biochemistry the subject was still considered an adjunct of physiology. The Biochemical Society had only just been founded though the *Biochemical Journal* had been started a few years before. There was no formal training in biochemistry as such to be had in those days, but Drummond was fortunate in his early years to come to know Dr O. Rosenheim and Professor W. D. Halliburton, both of whom exerted a profound and lasting influence upon him and of whom he continued to speak in later years of his life in terms of affection, gratitude and respect. Then, when he moved to the Research Institute of the Cancer Hospital, he learned much from Dr Casimir Funk, and also from the Director of the Institute, Dr Alexander Paine. It was under the care of Dr Paine that Drummond first made himself master of the methods involved in the feeding of diets of known composition to rats, methods which were to be of such great importance in his later investigations.

In July 1912 Funk, then working at the Lister Institute, London, had published a paper entitled 'The aetiology of the deficiency diseases' (*J. State Med.* 1912, **20**, 341), in the course of which the term 'Vitamine' was employed for the first time. This publication reviewed the existing literature concerning beri-beri, scurvy and pellagra, and, in advance of current views, suggested that pellagra, as well as scurvy and beri-beri, was to be regarded as a deficiency disease. In the previous year Funk had himself described (*J. Physiol.* 1911-12, **43**, 395) his pioneer attempt to isolate from rice polishings the substance capable of curing the polyneuritis which develops in pigeons fed on a diet of polished rice. Although his product was not pure, Funk believed it to be a basic nitrogenous substance, almost certainly an amine, and suggested the word 'vitamine' to describe the general class of substance of which the anti-polyneuritic substance was one example. 'Vitamines', according to Funk (*J. Physiol.* 1912-13, **45**, 75) were 'nitrogenous substances minute quantities of which are essential in the diet of birds, man and some other animals.' The fact that this courageous generalization appeared almost simultaneously with the classical experimental paper by F. Gowland Hopkins entitled 'Feeding experiments illustrating the importance of accessory factors in normal dietaries' (*J. Physiol.* 1912, **44**, 425), undoubtedly helped to turn widespread and serious scientific attention to the subject of accessory food factors or vitamins.

When, therefore, Funk moved to the Cancer Research Institute in 1913 and Drummond joined him the following year in the capacity of assistant, it was into a world of exciting new ideas in nutrition that the enthusiastic young man was introduced. Drummond's first task was to assist in a general chemical investigation of the phosphotungstic acid precipitate from rice polishings, the results of which were published jointly (2).

Hopkins and Neville had already shown (*Biochem. J.* 1913, **7**, 97) that rigorous purification of the diet of a rat was necessary for the demonstration of a deficiency of accessory food factors, and in their experiments the lactose used in the diet was several times reprecipitated by alcohol from aqueous solution. Taking up this point Drummond was able to show that lactose prepared from milk in the ordinary way contains important traces of an impurity which acts as a growth-promoting accessory substance in the rat (5). This paper confirmed in general the finding, previously observed by others, that no growth is possible in rats fed upon a purified diet which is deprived of the accessory food substances both water soluble and fat soluble.

At this time the importance of vitamins was not the only aspect of nutrition under investigation. The significance in animal nutrition of the amino-acid composition of dietary proteins was also being revealed, and Drummond decided to investigate the possible effects of dietary deficiencies of protein, amino-acids and vitamins on the growth rate of tumours in experimental animals, utilizing for this purpose rats inoculated with tumours of the rous sarcoma type. The results were negative in that no possibility was found of inducing an inhibition of a tumour growth by means of such dietary restrictions without very serious impairment of the nutritive condition of the host (10) but such a negative finding was clearly important.

When war came in 1914, Drummond found to his regret that his health did not permit his service in the Armed Forces. But he, together with Dr Alexander Paine, almost alone carried on the work of the Research and Pathological Departments of the Cancer Hospital, as may be seen from the Director's report for the year 1915.

Later in the war, when Halliburton was a member of the Food (War) Committee of the Royal Society, he made use of Drummond's experience in nutritional investigations on rats, inviting him to make comparative tests with butter and margarine and other butter substitutes, with reference to the fat soluble accessory food factors. This work was published with Halliburton in 1917 (7) and was extended and incorporated in the report of the Food Committee of the Royal Society, published in 1919 (15). There grew out of this work an investigation of the value of synthetic mannitol olive oil as food (19) which was published in collaboration with W. D. Halliburton and R. K. Cannan, also in 1919. In the war of 1939-45, Drummond was very conscious of being in historical succession to the Royal Society experts of the first war, and often quoted his old chief, E. H. Starling, who was a leading figure in the Committee of the first war.

As the result of these investigations Drummond had found an interest first in the water-soluble accessory food factors—vitamin B and vitamin C (9, 12, 16) and later in the fat-soluble accessory food factors—vitamin A and vitamin D (17, 18), an interest which remained for the rest of his life. His concern with the practical application of nutritional knowledge gained in the laboratory is illustrated in his paper entitled 'Some aspects of infant feeding' (14) published in 1918. Here he pointed out that the demand for accessory food factors was particularly evident during the period of growth, so that the protein and vitamin A of the tissues of the mother were made available to the infant at her expense if the diet was deficient in these substances. Furthermore, the young deprived of vitamin A not only failed to grow satisfactorily but were very susceptible to infection, particularly of the eyes. This was an early observation of the liability of a vitamin deficiency to reduce resistance to bacterial infection. Although he believed at that time that vitamin B was unlikely to be deficient in a country in which rice was not the staple cereal Drummond thought that human infants might well need other water-soluble factors, and endorsed the recommendation that orange juice should be given to artificially-fed infants. He pointed out that many substitutes for mothers' milk must, in many instances, be seriously deficient in both accessory factors 'A' and 'B', but particularly in the former. He attributed the danger of feeding skimmed condensed milk to infants not to its shortage of fat but to its lack of vitamin A, and recommended that cream or cod liver oil should be fed to meet a deficiency of this factor where it existed. He concluded his paper with the words 'It now remains for the practical worker on infant nutrition to apply the results of these experimental studies as soon as he is convinced of their accuracy.'

It was clear at this time that Drummond was becoming an acknowledged expert on nutrition and in 1918 he received the degree of D.Sc. of the University of London. It was, therefore, natural that he should be appointed to succeed Funk as Physiological Chemist at the Cancer Hospital in August 1918, but the next year, when R. H. A. Plimmer resigned the Readership in Physiological Chemistry at University College, London, on his appointment as Biochemist at the Institute of Animal Research, Bucksburn, Aberdeenshire (now the Rowett Research Institute), Drummond moved to University College, London at the invitation of Professor E. H. Starling. Here he was a Research Assistant from 1919 to 1920, and was then made Reader in Physiological Chemistry. In 1922, at the early age of 31, he was appointed to the newly created Chair of Biochemistry at the College, this appointment being made possible by a generous benefaction from the Rockefeller Foundation.

University College, London (1): 1919-30

By 1920 the number of accessory food factors or vitamins known to exist was three, fat-soluble A, water-soluble B and the antiscorbutic vitamin C. The realization that two types of factor, one fat-soluble and one water-soluble, were necessary for normal growth in the rat had clearly emerged from the experiments of McCollum and Davis in 1915 (*J. Biol. Chem.* 1915, **23**, 181). In 1918, E. Mellanby recognized rickets as a deficiency disease (*J. Physiol.* 1918-19, **52**, xi) and subsequently showed that the antirachitic vitamin was associated with certain fats. Then evidence gradually accumulated that two separable fat-soluble vitamins existed, one being antixerophthalmic and the other antirachitic (McCollum *et al*, *J. biol. Chem.* 1922, **53**, 293). The former was designated fat-soluble A or vitamin A, and the latter fat-soluble D or vitamin D. It was clear at this time that the original concept of 'vitamines' as nitrogenous bases was inadequate to account for the existence of the fat-soluble apparently non-nitrogenous accessory food factors, and in 1920 Drummond made the suggestion (22) that since the termination 'in' implied, in accordance with systematic chemical terminology, a substance of unknown constitution, the terminal 'e' of Funk's neologism 'vitamine' be dropped. The new term 'vitamin' was widely adopted and in time completely replaced the former cumbrous expression 'accessory food factor'.

From 1920 onwards, as his own master of research, Drummond rapidly broadened his interests. Indeed in retrospect one may well gain the impression that he cast his net somewhat too widely. Thus between 1920 and 1925 his publications are concerned not only with the properties and nature of vitamin A and vitamin B but also with the formation of vitamin A in plant tissues (31) and in a marine diatom (42), the relationship of fat-soluble vitamins to rickets and growth in pigs (30), the effect of vitamin B deficiency on reproduction (62), the use of cod liver oil in the feeding of farm animals (47, 48), nutrition and growth on diets devoid of fat (21, 33) and the nature of an alleged preparation of the anterior pituitary growth hormone (40). His interest in practical affairs took him to the Norwegian cod-fishing fields in order to investigate the manufacture of cod liver oil and of the liver oils of other fish (34, 36, 37), and he wrote also on the selection of cod liver oils for medical use (63). But a broad sweep of canvas appealed to the artist in Drummond, and indeed he has been described as 'like human quicksilver' because of the way he darted from one thing to another, from one place to another, and from one problem to another, with almost childish exuberance and enthusiasm, which was in fact contagious, particularly to the younger men around him. He had a quick, impulsive, creative mind, whose main interest was in the generalities of broad questions; elaborate detail bothered him as a modern necessity. He disliked the careful building up of a vast structure of supporting evidence, preferring to rely upon his own wide knowledge and experience in reaching a conclusion. It was for this reason that his contributions to the advance of knowledge of the chemical nature of vitamins, depending as it did upon the laborious accumulation of detailed evidence, were of much less significance than his influence on the development of nutrition as a whole. His achievement in building up a school of young biochemists at University College was of especial significance to the growth of biochemistry in Great Britain and must remain as the mark of a man who, in this respect, might well be envied by many to whom academic laurels came in greater profusion.

The Department of Biochemistry which Drummond created at University College was small and during his time was a part of a larger department of Physiology and Biochemistry, but under Drummond's influence it soon became an important centre of teaching and research, and exerted an influence on the development of the growing subject of biochemistry out of proportion to its size. Although most of his own work was concerned with some aspect of nutrition, Drummond encouraged his more senior pupils to develop independent lines of work, and thus the interests of the Department rapidly widened. He believed it to be an important duty of a Professor to train men for senior posts elsewhere, and in this he was singularly successful, infusing some of his own breezy vitality and energy into those around him, and meeting with so much success in the training of pupils that no fewer than nine of those who were his colleagues or pupils hold or have held University Chairs. No other School of Biochemistry in Great Britain has produced so many Professors as did his Department during Drummond's tenure. The secret of his success in this is hard to elucidate. It was Drummond's own habit to work hard and to play hard, and he did both with an enthusiasm which he readily imparted to those around him. His energy was certainly a most striking characteristic. Although in the 1920's he was carrying on much consultant work for industry, he could always find time to discuss problems with a junior research worker, and even though he might not be much concerned with details, he would be able to make startling and sometimes valuable suggestions concerning the possible general developments of the work in hand which, for the man who was capable of responding, were of great stimulating force.

Drummond was a good teacher and his lectures on his special subject of nutrition were outstanding. His teaching also dealt most entertainingly with that aspect of biochemistry concerned with the inter-relationships of enzyme systems within the cell (see 75). It was natural that the junior members of the Department should attempt to emulate their Professor in the production of fluent and interesting lectures, and here again Drummond's impress on his colleagues was both widespread and long-lasting, and of some significance in the development of biochemistry as a whole. But his influence at University College was by no means confined to those who sought to specialize in biochemistry. Drummond himself was particularly good at getting over to a lay audience some understanding of the complexities of science, a gift which is given to few. He also threw himself whole-heartedly into undergraduate affairs and was a popular speaker at student societies of all sorts. He was a co-founder of a dining club at which students and staff could meet informally, which still flourishes as a memorial to his interest in the young man. In many a cricket match at University College, serious and less serious, Drummond was a zealous and apparently indefatigable player. As a Professor, and a teacher in the widest sense of the word, Drummond was outstanding in a college whose staff has never lacked brilliance and originality.

It was during his early years at University College that Drummond developed an important colour reaction for vitamin A in collaboration with his old master and friend, Dr O. Rosenheim. In 1920 Drummond and Rosenheim suggested that the chromogen responsible for the transient purple colour obtained when cod liver oil is treated with sulphuric acid might be related to the vitamin itself (20), and subsequent investigations appeared to bear out this view (39). In 1925 Drummond and Rosenheim described the more sensitive and less transient colour reaction given by liver oils with arsenic trichloride, and provided evidence which strongly suggested that there must be a relationship between the chromogen and the vitamin (65). It was perhaps unfortunate for Drummond and Rosenheim that when, in the following year, Carr and Price replaced arsenic trichloride by the less objectionable antimony trichloride, the colour reaction should have been widely adopted under the name of the Carr–Price reaction. In 1929 Drummond, with R. A. Morton of Liverpool, was able to provide clear evidence that the quantitative use of the Carr–Price reaction was a reliable means of determining the vitamin A potency of cod liver oils (81).

During these early years of research on vitamins more than one worker called attention to the apparent relationship between the presence of yellow pigment in vegetable foodstuffs and the activity of the latter as a source of vitamin A. Steenbock had observed this association in a number of natural foodstuffs in 1919, but had pointed out that certain exceptions occurred in which the presence of the vitamin was not associated with yellow pigment; he suggested that the colouring matter might sometimes be present in the form of a leuco-compound. Drummond and Coward (25), as the result of a survey of the distribution of vitamin A, concluded that 'unless we assume the existence of a leuco form, it does not appear probable that the fat-soluble vitamin is a member of the lipochrome class of pigments. The frequent association of the growth factor with pigments of that type must therefore be regarded as accidental.' Nevertheless, Drummond, in collaboration with O. Rosenheim, prepared carotene, the yellow colouring matter from carrots, and found that it was without effect in the growth tests then in use for the detection of vitamin A activity (20). When subsequently it became clear that in these experiments the animals had been suffering from a deficiency not of vitamin A alone, but of both vitamin A and vitamin D, Drummond reopened the question of the possible vitamin A activity of carotene, concluding once again that carotene was without vitamin A activity (84). This conclusion was unfortunately incorrect, and subsequently the vitamin A activity of carotene was confirmed by Drummond and his colleagues (89), the earlier negative results being ascribed to the fact that the solvent, ethyl oleate, employed as a vehicle for the administration of the carotene to the deficient rats, tends to undergo oxidation, particularly when it is purified. This oxidation was found to involve the dissolved substance, especially when the latter was pure, and the authors suggested that the impurities normally present both in ethyl oleate and in carotene could serve as antioxidants. With characteristic generosity Drummond took the entire responsibility for this error on to his own shoulders at a meeting of the Biochemical Society.

During the 1920's Drummond and his colleagues made a number of attempts to isolate pure vitamin A from fish liver oils, and finally, in 1932, in collaboration with Heilbron and Morton of Liverpool (98), he produced highly concentrated viscous liquids which were more active than the purest carotene then available. Although the products were not absolutely pure, some of them can now be seen from their properties to have contained about 80% of a mixture of the two vitamin A isomers plus 4 to 8% of vitamin A_2. Drummond was perhaps unfortunate in having chosen to attempt the isolation from natural sources of a substance as notoriously difficult to purify and crystallize on the laboratory scale as vitamin A turned out to be. Drummond's subsequent investigations on the purification and nature of vitamin E were similar in result. A study of the unsaponifiable fraction of wheat-germ oil led to the preparation of an oily product which, although not demonstrably homogeneous, appeared to be substantially more active with respect to vitamin E than any previously obtained (121, 125). After the isolation of α-tocopherol from wheat germ oil as a crystalline allophanate by Herbert Evans and his collaborators (*J. biol. Chem.* 1936, **113**, 319) it appeared that the product obtained by Drummond and his colleagues must have been the vitamin in a state very nearly pure, although unfortunately there was no evidence of its purity at that time. Already, however, before this work on vitamin E was completed, Drummond's attention had been turning more and more to an interest in practical nutrition, a matter that had clearly been in his mind from the time of the paper on infant feeding (14), published in 1918, to which we have referred earlier.

University College (2): 1930-39

In 1929 Drummond made a visit to Newfoundland on behalf of the Empire Marketing Board to study the cod liver oil industry there, a report on which was subsequently published with Professor T. P. Hilditch (91). Although the writing of this report did not escape some criticism (see *Nature, Lond.* 1931, **127**, 631) the information in it was of great practical value in directing attention to a plentiful source of high potency liver oils within the British Empire, a matter of some economic significance at this time. During his visit to Newfoundland Drummond became interested in the nutritional condition of the inhabitants, particularly the fishermen, whom in some instances he believed to suffer from vitamin deficiencies caused by a lack of the very vitamins which were abundant in the products they prepared. When he was invited to Stanford University, California, to give the Lane Medical Lectures in 1933, he published these under the title 'Biochemical studies of nutritional problems', a wide survey of the subject and an indication of Drummond's growing interest in nutrition in general (116). In 1933 he also visited Spain to lecture on nutritional subjects at a time when revolutionary fighting was in progress, these lectures being delivered under the aegis of the League of Nations Health Organization. Drummond had been interested in the 'Peoples' League of Health' from its inception, this having been founded in

1917 by the late Miss Olga Nethersole, C.B.E., to offer general advice on health, and it was natural that Drummond should be consulted in matters of nutrition and health. His conspicuous ability to convey to a lay audience the practical significance of the technical aspects of nutrition, and the role played by vitamins in health and disease, led to his being called to advise many bodies concerned with nutrition, and he was greatly in demand as a lecturer both in Great Britain and abroad.

Indeed, in Great Britain in the early 1930's a new interest in nutrition was developing, undoubtedly fostered by the economic crisis of 1931. In that year a Committee of the Ministry of Health had been set up to advise on general questions of nutrition, while in 1933 another Committee was set up by the British Medical Association with similar interests. In 1936 the report by John Boyd Orr (later Lord Boyd-Orr) on *Food health and income* (MacMillan London, 1936, 2nd Ed. 1937) revealed the inadequacy of the diets of families with low incomes, and showed that the standard of health of these families, especially that of the children in them, was much lower than that of people with higher incomes. The tentative conclusion reached was that 'a diet completely adequate for health, according to modern standards, is reached at an income level above that of 50 per cent of the population'.

In 1934 Drummond's own interest in the practical problems of nutrition was quickened in rather a striking way. In the previous year the late Professor H. E. Armstrong, in his capacity of a Governor of Christ's Hospital, had become aware that Dr G. E. Friend, the Medical Officer of the School, had much information that might be of general interest concerning the health of the schoolboy and his nutrition, during the first world war and after. As a result of Armstrong's suggestion, Drummond with his friend, Dr J. Vargas Eyre, visited Christ's Hospital early in 1934, and the writer well remembers the excitement with which Drummond returned to the laboratory, having found that Dr Friend's figures suggested that the incidence of fractures at Christ's Hospital rose when butter was replaced by margarine during the latter part of the 1914-18 war, and rapidly fell again when butter was restored in 1922. As the result of the interest of Armstrong, Eyre and Drummond, Dr Friend wrote a book on this subject, *The schoolboy—his nutrition and development*, which was published in 1935 (Cambridge: Heffer), and to this book Drummond contributed a preface, the first sentence of which reads 'It is a fact at once surprising and humiliating that with thousands of years of human life and experience behind us we are actually engaged today in acquiring laboriously the knowledge necessary to enable us to feed and rear our children properly.' In this preface Drummond emphasizes how often malnourishment was seen in growing boys of thirteen or fourteen in past years, and how often the cravings of natural hunger must have been regarded as a manifestation of the carnal sin of greed, to be suitably repressed. In truth, as he said, a growing boy of thirteen or fourteen years requires as much food as a man who is doing a fair day's work. His comments are a significant foreshadowing of the firm position he later took in the Ministry of

Food on the desirability of making available to the nation a diet which was unrationed with respect to total calories.

In 1935 Drummond's wish to study nutrition in other countries became known to the Rockefeller Foundation and as a result he was given a special short Fellowship by the Foundation for the period January-May 1936. During this period he visited Holland, Germany, Czechoslovakia, Austria, Hungary, Switzerland, Poland, Russia, Finland, Sweden, Norway and Denmark, in order to survey nutrition in these countries, and particularly the nutrition of the poorer people. He came back full of enthusiasm for the experiments in canteen feeding which he had seen in Eastern Europe, and his experiences at this time undoubtedly influenced his views when he assumed some degree of responsibility for advice on nutritional matters in Great Britain in 1940. At this time he had become convinced of the unfortunate results, particularly with respect to nutrition, which the industrial revolution had brought about in Great Britain, and it was this developing interest in historical aspects of nutrition which led ultimately to the writing of *The Englishman's food* (156, see also 126, 148 and 155). Although his growing concern with practical problems of nutrition involved the directing of much of his activities into pathways of this sort (117, 131, 132, 137, 138, 140, 153) he still continued investigations on the chemical nature of vitamin E (135, 136, 139, 141, 145), the relationship of vitamin deficiencies to the endocrine system (144, 150), and other matters of general biochemistry (143). Nevertheless, in 1938 and 1939 his interests were being more and more taken up by historical studies of English diet and nutrition.

The Englishman's food, a history of five centuries of English diet, by J. C. Drummond and Anne Wilbraham (1939), was published by Jonathan Cape at a most opportune moment (156). The work is generally regarded as a scholarly contribution to history, and it includes much material that had not previously been brought together in one book. It starts with a discussion of medieval agriculture and the production of food in medieval and Tudor England. A discussion of the 17th and 18th centuries leads to a consideration of the influence of the industrial revolution and the deterioration of physique found during the 19th century. In the final chapters, dealing with the 20th century, the development of nutritional research, its application during the war of 1914-18, and the opening of what is described as a new era in the period of 1919-39 is discussed with the conclusion that 'It is fair to say that there is no problem of nutrition in England today. So much research work has been done in the laboratories and so many precise dietary surveys have been made that we know all we need to know about the food requirements of the people and the extent to which they are or could be provided. The real problem is how to make it possible for those whose health is being adversely affected by a faulty diet to obtain not only the knowledge about the right kinds of food to eat but the food itself. One thing is certain, a means must be found of bringing essential food within the reach of the poorest section of the community. How this is to be done is not yet apparent, but it is to be hoped that the problem will be faced in a frank and honest manner.' How much Drummond himself would be able to assist in solving this problem was, of course impossible even to guess at when these words were published early in 1939.

The publication of *The Englishman's food* and the outbreak of war were not the only turning points in Drummond's career in the fateful year of 1939. To the sorrow of his many friends his first marriage broke up in that year, and in the following year he married Anne Wilbraham, who had been his secretary for many years and who was his co-author in *The Englishman's food* and in an earlier historical essay (126).

The Ministry of Food: 1939-46

When war came on 3 September 1939 Drummond, like many others of those willing to help, had no obvious niche in the colossal machine which had to be constructed in order that the war might be won. In June 1939, with some other Professors, he had received a letter from the Provost of University College informing him that if war broke out the College would be moved to Wales and that as far as could be foreseen his services would not then be needed. It was made clear in this letter that he would remain a member of the College staff, and that his salary would be forthcoming, but the College itself hoped that he would be able to undertake some form of National Service.

Already, in September 1938, Drummond had offered his services to the expected Ministry of Food should war become a reality, but at this time it was suggested that if a Ministry of Food were brought into being as the result of an emergency it would look to already established organizations for advice on the scientific and technical aspects of problems concerning food. There was then no intention that a Ministry of Food should subsume a scientific staff.

Soon after the outbreak of war Drummond was consulted informally about gas contamination of food, and on 16 October 1939 he was officially appointed 'Chief Adviser on Food Contamination' to the Ministry of Food. At first his duties were largely advisory, reliance being placed on the co-operation of existing research organizations for the carrying out of any needed investigations but as the result of his activities a manual on *Food and its protection from poison gas* was published by the Ministry of Food in 1940 (H.M. Stationery Office S.O. No. 70-9999). Early in that year he visited Porton Experimental Station to collect information, and to make experiments on decontamination and on standards of fitness of foods exposed to gas contamination.

Now that he was in the Ministry Drummond began to interest himself in a number of scientific aspects of its work and shortly submitted a memorandum in which he criticized as inadequate the available machinery for dealing with new processes and inventions that might be useful in war-time. But it is clear that he did not find his position one which provided full-time occupation, and he chafed somewhat at the periods of inactivity with which it was associated. At this time he was able to keep some academic research going, despite the closing of University College, at the Courtauld Institute of Biochemistry, Middlesex Hospital, London, by courtesy of the Director, Professor E. C. (now Sir Charles) Dodds.

On 13 December 1939 Drummond and A. L. Bacharach read a paper to the Food Group of the Society of Chemical Industry, which was subsequently published, entitled 'The composition of optimal and marginal diets' in which the evidence for human nutritional requirements was critically examined and in which ideas later to be accepted as of importance in the Ministry of Food were discussed (160). But at that time the place in the planning of a nation's food supply of a scientist at the Ministry of Food was not yet clear to those in administrative authority. Early in January 1940 Drummond was called to a conference by the Parliamentary and Scientific Committee to arrange a meeting at which war-time bread would be discussed from a scientific point of view. It was agreed at this preliminary meeting that Dr J. Vargas Eyre, who had raised the question, should invite independent scientists to contribute to the discussion, together with representatives of the Flour Milling Industry and the Baking Trade; the subject of discussion would be the most suitable national loaf for war-time. When it was proposed that the interested Ministers should be represented by their Scientific Advisers, it became apparent that the Ministry of Food had no Scientific Adviser. A number of M.P.s agreed to take up this matter at once, but when the meeting of the Committee was held on 31 January 1940, Drummond stressed that he was speaking in his private capacity, and indeed on that day there was no post of Scientific Adviser to the Ministry of Food in existence. At this meeting he stated that in his view the country would not reach a maximal nutritional state until something approximating to wholemeal bread was made available, particularly to the poorer classes. He did not regard meat as of such importance as the availability of ample supplies of wholemeal bread and vegetables, though he was not making a plea for vegetarianism. Physiologically, in his view, fats were not an important part of the diet provided that green vegetables were plentiful. He made clear that the country was faced with two questions, first how to increase the consumption of dairy produce and vegetables, particularly among the lower income levels of the community, and second how to obtain wholemeal flour. He thought that both these aims could be achieved and although it would not be possible to do everything in this direction at once, he believed that the consumption of potatoes and vegetables could be stimulated very greatly during the war period. Drummond's views on the importance of wholemeal bread, supported as they were by the representative of the Ministry of Health, did not pass unchallenged by representatives of the Flour Millers. While expressing willingness to do whatever was deemed best in the circumstances they pointed out that the 25 per cent taken from the wheat in the preparation of 75 per cent extraction (white) flour was of great importance as an animal foodstuff; they agreed, however, that this loss ought to be considered with respect to the fact that a smaller amount of wheat would then be needed for flour.

On the same day (31 January 1940), Drummond submitted to the Ministry of Food a memorandum 'On certain nutritional aspects of the food position' in which he reviewed the pre-war nutritional position of Great Britain and the probable effects on it of war-time conditions, particularly stressing the effects on the poorer sections of the population. He put forward the view that bread should be of a high nutritive value, that the consumption of potatoes, oatmeal, cheese and green vegetables should be increased, that not less than one pint of milk a day should be supplied to expectant and nursing mothers and to children up to the age of 15, and that margarine should be fortified with vitamins A and D (cf. 162). He again expressed the opinion that a restriction of the consumption of meat would be relatively unimportant, particularly if supplies of cheese and fish were adequate (cf. 161).

On the following day (1 February 1940) Drummond was officially appointed Scientific Adviser to the Ministry of Food. In this position he needed to utilize to the full the blend of qualities which went to make up the man as he was. His knowledge of and experience in laboratory research stood always in the background. An unusual sense of history, illustrated in *The Englishman's food*, was combined with a good general knowledge of modern nutritional developments and needs and a flair for exposition of such knowledge to lay audiences. His ability to express scientific findings in words easily understood by the layman was of great importance, as also was his capacity to become easily acquainted with all classes of men. The boyish vigour and breezy and approachable manner which characterized him from the earliest days did not desert him even during the darkest days of war. At times when the Ministry of Food was subject to much criticism, he dealt quickly and firmly with a host of day-to-day problems, while at the same time advising the Minister wisely and effectively on matters of long-term policy.

His position as Scientific Adviser was not a ready-made one, with clearly defined boundaries. The Minister of Health was charged with the responsibility of advising the Government on nutritional policy, while to the Minister of Food was assigned the task of giving effect to the policy adopted, the Medical Research Council being available for special advice. Not himself a medical man, and one who at times had not concealed his critical attitude to medical doctors for what he thought to be their deplorable lack of knowledge of the scientific bases of nutrition, Drummond might well have been regarded as a difficult collaborator by his medical colleagues. But, in fact, as Lord Horder has written of him, 'his tolerance, his humility and his insatiable desire for the truth never failed him, and he was for this reason the ideal colleague' (*Brit. Med. J.* 1952, **2**, 395). He was indeed at pains to work harmoniously with his colleagues in other Government departments and elsewhere and it was largely at his instigation that a Standing Interdepartmental Committee was set up in 1941 on Medical and Nutritional problems to which all interested bodies sent representatives. This Committee did much useful work behind the scenes and its decisions were reached only after all relevant points of view had received full consideration. It was in such planning that Drummond excelled, and since his fearlessness and vitality were associated with a friendliness that disarmed those whom he criticized, he was able to secure the fullest collaboration with his scientific and administrative colleagues. His approach to problems was often empirical rather than sprucely academic, and if something did not work out as he hoped he sometimes did not hesitate rapidly to change his tactics.

At the time of Drummond's appointment as Scientific Adviser, the Ministry of Food was still relatively young, though with the coming of Lord Woolton as Minister on 4 April 1940 maturity was rapidly attained. When Lord Woolton was first appointed he was necessarily much occupied with practical problems of immediate importance in which his own industrial experience was of great value. Nevertheless the new Minister was fully aware of the value of scientific advice in the affairs of the Ministry, and his first inquiries included the question 'Where is the Scientific Adviser to this Ministry?' Drummond himself related how he introduced himself to the Minister by first sending him a copy of *The Englishman's food*, and it appears that the Minister having rapidly appraised the importance of the book was equally quick in his appreciation of the man and of his special abilities. Much depended upon the establishment of a full mutual trust between Adviser and Minister despite the difference of their background and their different approach to their common problems.

In an account of Drummond's activities at the Ministry of Food it is clearly impossible to extricate precisely what was his share in the complex developments that went on, but in the overall result at the highest levels can be discerned threads which Drummond had already begun to spin before the war. Thus in 1940 the Lord Privy Seal, in his capacity of Chairman of the War Cabinet Committee on Food Policy, set up a Scientific Food Policy Committee of which the Chairman was the President of the Royal Society. This Committee had as one of its principal functions to advise on the minimal diet required to maintain health and efficiency under war conditions, with due regard to the potential availability of supplies from home and overseas, and to questions of labour, shipping space, foreign exchange and other economic matters. This Committee, in the course of its work, considered among others the following problems: collective feeding; the economic and nutritional aspects of potatoes, bread and flour; supplies of fats and oils; the use of dried milk; the adequacy of supplies of protein; and the availability of vitamin concentrates. In the consideration of all these matters the Ministry of Food and its Scientific Adviser's Division necessarily played an important part.

At all times Drummond was able to blend skilfully into a clear and easily read document the results of discussions with economists, statisticians, agriculturists and other specialists, and in so doing was able to guide the thoughts and decisions of many Committees of different types.

Shortly after Drummond's appointment he made strong recommendations against any proposal to reduce the total consumption of food in the country, emphasizing not only his own practical experience, but also that in the 1914-18 war, when, as he said 'most unwisely and against expert advice a campaign urging voluntary restriction in the consumption of foods, particularly bread, was launched by the Government', so that 'those who loyally responded suffered loss in weight and reduction of efficiency'. His great success in assessing the over-all calorie needs of a nation at war, and in assisting to provide unrationed foods, chiefly bread and potatoes, to ensure that individual variations in the total calorie requirements could be covered, may be gauged from the pamphlet *Food consumption levels* published in 1944 in Great Britain by the Ministry of Food (S.O. No. 70-444*) and from the lectures delivered by Drummond himself in later years (176, 177, 178, 179, 180, 181).

In May 1940 the Ministry of Food's import programme for the second year of war carried an appendix entitled 'A survey of war-time nutrition', setting out what were in fact Drummond's views on the nutritional policy that ought to be adopted. It was the first time that modern knowledge of nutrition could be applied to the problem of the war-time planning of the food supply of a nation which was dependent on imports for about two-thirds of the energy value of its food supply. The survey showed that before the war the nation's food had been adequate in what were described as energy-rich foods, but short in what were called protective foods, particularly in those supplying calcium, vitamin A and vitamin B_1. During 1940-41 home production was expected to provide about one-third of the nation's energy requirements, two-thirds of its needs for calcium, one-third of the necessary vitamin A, two-fifths of the vitamin B_1 and, as a consequence of a rise in home potato production, all the necessary vitamin C. Recommendations were made for improving the supply of vitamin B_1 either by increasing the extraction rate of flour or by fortifying white flour with the synthetic vitamin. The expansion of home production of milk and vegetables including potatoes was advocated, while the need was emphasized for the increased importation of cheese, dried or condensed milk, canned fish, pulses, and of concentrates of vitamins A and D with which margarine could be fortified (see 162, 163, 165, 166). It was further recommended that the importation of fruit (other than that of oranges), nuts, and eggs in shell, should be reduced, since such imports were wasteful of shipping space when considered in relation to either energy production or protection. The impossibility of reducing supplies of energy-rich foods without the risk of impairment of the nation's health and vigour was again stressed. The classification of foodstuffs into 'energy-rich' and 'protective' was a concept at once simple and effective. Another valuable and simple idea was that of 'vulnerable groups' which covered pregnant women, infants and so on.

Drummond's value was now widely recognized and in the Fourth Report of the Select Committee on National Expenditure (7 May 1940) the expansion of the scientific staff of the Ministry of Food was recommended. One important result of this recommendation was the appointment of Dr T. Moran as Drummond's deputy in the following month.

In September 1941 Drummond submitted to the Ministry an important memorandum entitled 'The nutritional position in the United Kingdom after two years war'. This report related a series of nutritional levels, 'Spartan', 'Subsistence', 'Adequate' and 'Ample', to the needed tonnages of food imports, and referred to the menacing outlook at the beginning of 1941, particularly with respect to protein and calcium for growing children, and to vitamin A, which was 'sharply relieved by the liberal supplies offered by the U.S.A. under the terms of the Lease-Lend Act'. The report also pointed out that a considerable proportion of adults lost weight during the winter and spring of 1940-1. On the basis of a requirement of 2900 calories for each person, a matter which at that time was in debate, a 'spartan' regime would require greatly increased home production of food, but could reduce the import programme to 9 to 10 million tons, while a subsistence diet could be devised with an import programme of 11 to 12 million tons. In fact, during 1944, the import programme was 11 million tons compared with 22 million tons per annum before the war (176). Thereafter each year until 1946, when he left the Ministry, Drummond circulated reports of this type, which not only explained to his non-scientific colleagues the principles on which the nutritional requirements to be met by the expected import tonnages should be considered, but also discussed their application to particular problems.

Much of Drummond's success at the Ministry was undoubtedly due to his persuasiveness and the skill with which he managed Committee work. He was constantly involved in discussions with the other bodies about the establishment of appropriate levels for the vitaminization of margarine, about nutritional targets for school meals, the relative nutritional value of rations distributed to different types of factory canteen, the establishment of standards of vitamin content for orange juice and the organization of propaganda to explain the importance of fruit juices in infant feeding and the establishment of the average need for calories 'moving into consumption' for the population as a whole. These and many other negotiations, often about matters more for judgment than for pedantic ratiocination, were carried through with the urbane assurance of a man who seldom lost his intuitive grasp of the wider principles involved, but who still possessed an almost schoolboy delight in new ideas, from whatever source they might be derived. Another most valuable asset, unusual in a Professor, was his facility of eliminating non-essential details from his recommendations; he would convey in a few succinct paragraphs the fundamental reasons for the actions he advocated. If the necessity arose he would fearlessly plunge into the minutiae for the benefit of his scientific collaborators, but to his non-scientific administrative colleagues his terse lucidity was a god-send.

Drummond regarded the war as an opportunity for attacking dietetic ignorance, and of changing the nutritional status of vast numbers of people whose condition had been the object of his criticisms before the war. And although he was but one wheel in the complex machine which achieved this aim, his own views, and his ability to put them in the right form, at the right time to the right people, provided a most significant contribution to the solution of a problem which was almost staggering in its immensity. He was particularly interested in the provision of special supplements for mothers and children. Jointly with his colleagues in the Ministry of Health he played a full part in organizing supplies of cod liver oil and vitamin tablets for expectant and nursing mothers. He helped too with the cheap supply of liquid milk to 'vulnerable groups' and in their priority when milk was rationed, in the early experiments with black currant syrup and rosehip syrup as sources of vitamin C for expectant mothers and young children, in the subsequent provision of concentrated orange juice and cod liver oil to these two groups when such preparations became available, in the generous allocation of rationed food for school meals, and in the provision of national milk and cocoa for adolescents. Many of these arrangements were maintained after the war and even with the cessation of rationing in 1954 some of them are to continue. Is it too fanciful to trace a line of thought through *The Englishman's food* to the decisions which made possible the post-war continuation of these supplements?

The development of dehydrated food as a means of conserving shipping space was another of Drummond's interests and he initiated the importation of dried egg into this country, together with large quantities of skimmed dried milk. He also assisted the widespread distribution of dehydrated soups and vegetables.

One of the most difficult problems on which Drummond had to advise during his time at the Ministry was the subject of bread. As he pointed out (180) when supplies of most staple foods are restricted by rationing or by other causes the balance of individual energy requirements can be easily provided only if there is available an unrestricted pool of calories. It is important that the foodstuffs constituting this pool be cheap, and the provision of this in the form of unrationed bread and potatoes was a cardinal principle of food policy during the war. But the supply of the vitamins B ought to be related to the total carbohydrate intake, as had become clear from academic research carried out in the pre-war period. If this view be accepted one is forced to conclude that the low extraction type of white flour eaten generally in Great Britain before the war was, in itself, nutritionally defective, since much of the B vitamins, present in the wheat berry, were removed in the process of milling. When bread and other carbohydrate-containing foods such as potatoes came to occupy an increasingly significant dietary position during the war, the need to find additional sources of the B vitamins became of paramount importance. In his statement to the Parliamentary and Scientific Committee on 31 January 1940 Drummond had strongly advocated the introduction of wholemeal bread and in April

1940 he advised that a wheaten flour of 82 to 85 per cent extraction should be provided rather than the white flour of 70 to 75 per cent extraction enriched with synthetic vitamin B_1, which it was then proposed to make available. A month later, the Medical Research Council presented a memorandum published in August (*Lancet* 1940, **2**, 143) recommending bread based on a flour of 80 to 85 per cent extraction. On 18 July 1940 the Ministry of Food announced that two kinds of bread would be provided at the same price: a loaf baked from white flour of 70 to 72 per cent extraction and enriched with synthetic vitamin B_1, together with an unspecified quantity of a calcium salt; and a 'wholemeal bread' (see 163). The addition of calcium, on the recommendation of the Medical Research Council, was made to ensure that all classes of the population received at least the daily requirement (163). However, in the spring of 1942 it was decided to enforce the consumption of bread made from flour of 85 per cent extraction, with the total exclusion of the white vitamin-enriched loaf. The relatively dark 'National wheatmeal' loaf based on the flour of 85 per cent extraction did not meet with an enthusiastic reception, but technical developments improved the quality of the flour in time, rendering it lighter in colour. In the autumn of 1944 the extraction rate was reduced from 85 per cent to 82½ per cent and then to 80 per cent. In these developments Drummond had the powerful co-operation of Dr T. Moran, Director of the Flour Millers' Research Association, who had been seconded to the Ministry in June 1940, and his assistance was most valuable in dealing with the specialized questions involved. Even today the controversy about the extraction-rate of flour is not concluded, but the fact that, despite all difficulties, agreed conclusions were successfully put into operation during the stresses of war is a tribute to all concerned, and particularly to the Ministry of Food, the Ministry of Health and the Medical Research Council.

In order to assess the effects of changing conditions the Ministry of Food began, in 1941, dietary surveys in households in eighteen representative areas in the country, while from 1943 onwards, surveys of body-weight were also carried out. The Ministry of Health made comparable clinical surveys, and as a result a close check was kept on the condition of this representative sample of the population. The results (cf. J. M. Harries and D. F. Hollingsworth (*Brit. Med. J.* 1953, **1**, 75)), provided gratifying support for the policies of the Ministry of Food during the dark days of war and potential deficiency.

In 1947 the 'Lasker Group Award' was presented by the American Public Health Association to the British Ministries of Food and Health, 'for the unprecedented program of food distribution in Great Britain, with resulting improvement in the health of the people'. The citation for the award gives graphic and independent evidence for the high value placed abroad upon the activities of the leaders in this great enterprise.

'Long before World War II, the Medical Research Council and the health authorities of Great Britain had been active in promoting research and in spreading knowledge of nutrition among the people.

'In 1937 an Advisory Committee on Nutrition was set up by the British Ministry of Health which undertook a survey of the national diet. The background had therefore been developed before the outbreak of the war for the first large-scale application of the science of nutrition to the population of the United Kingdom. A separate Ministry of Food was established with wide executive powers over the production and rationing of foods, the purchase of foods from abroad and the education of the public in the proper use of available foods.

'By the effective employment of its great powers the Ministry of Food, in consultation with the Ministry of Health and with the advice on all matters which might affect the health of the people of a Standing Committee under the chairmanship of the Chief Medical Officer, succeeded to a remarkable degree in providing a diet for all the workers of the country in conformity with their physiological requirement, irrespective of income.

'Although almost all other environmental factors which might influence the public health deteriorated under the stress of war, the public health in Great Britain was maintained and in many respects improved.

'The rates of infantile, neo-natal and maternal mortality and stillbirths all reached the lowest levels in the history of the country. The incidence of anaemia and dental caries declined, the rate of growth of schoolchildren improved, progress was made in the control of tuberculosis, and the general state of nutrition of the population as a whole was up to or an improvement upon pre-war standards.

'In the opinion of the Lasker Awards Committee, this has been one of the greatest demonstrations in public health administration that the world has ever seen. The Lasker Awards Committee of the American Public Health Association therefore takes great satisfaction in recommending awards for scientific and administrative achievement to the British Ministries of Food and Health and to the four great leaders in this historic enterprise, Lord Woolton, Sir Jack Drummond, Sir Wilson Jameson and Sir John Boyd Orr.'

During the war Drummond was involved in the necessity of much travelling. Towards the end of 1941 the nutritional situation in Malta had become serious, and Drummond was asked to send somebody to look into it. Characteristically, he went himself, early in 1942, and was able to make a number of important practical suggestions, including a requisition for one cwt. of ascorbic acid to be sent in by air. He himself was flown out of the island in a lull between air raids. When food supplies began to flow from the United States under the Lease-Lend Act, Drummond had to make many transatlantic visits. He was one of the delegates to the Hot Springs Conference, in June 1943, which led to the formation of the Food and Agriculture Organization of the United Nations, whose basic purpose is to raise the levels of nutrition throughout the world. He gave an interesting account of this meeting at a subsequent lecture (172).

In 1944, with the end of the war in sight after the invasion of the Continent, Drummond became adviser on nutrition to SHAEF and to the Allied Post-

War Requirements Bureau. At that time the Council of British Societies for Relief Abroad was concerned about the health of the people of the countries then being liberated or whose liberation was imminent. It set up a Committee of which Drummond was Chairman, to write a short manual for the use of relief workers, to help them assess the nutritional state of the population liberated, and to take practical steps to improve it quickly and safely. The resulting booklet, entitled *Nutrition and relief work*, published in January 1945, owed much to Drummond's commonsense and lucidity. It was widely used as a standard text-book by all the organizations working in liberated countries. In May 1945 Drummond was a member of a party of medical and scientific people who were allowed to pass through the German lines into occupied Holland before hostilities had ceased, in order to advise on the procedures needed to relieve the starving Dutch people with the minimum of delay, and not only did he speedily bring succour to the starving but also ensured that the unrivalled opportunities for the investigation of scientific aspects of starvation thus presented should be utilized to the full (173, 175, 177, 179, 181). It was natural that, with all his experience Drummond should be appointed as Adviser on Nutrition, Control Commissions for Germany and Austria (British Element) during the period immediately after the war (1945-6).

Even with all these many affairs on his hands during the period of the war Drummond still had the energy and ability to undertake other duties. From 1941 to 1944 he was Fullerian Professor of Physiology in the Royal Institution, and during his tenure gave four courses of afternoon lectures on nutritional subjects, and two Friday evening discourses. Unfortunately of all these only one was published—the discourse on the Hot Springs Conference (172). In 1944 he became Chairman of the Committee on Hospital Diet of King Edward's Hospital Fund and under its auspices two memoranda were published on hospital diet, the second in 1945. These memoranda, based in part on dietary surveys carried out under Drummond's supervision, marked the beginning of an intensive and effective campaign for the improved preparation and service of meals in hospitals, a matter which, in some instances had been surprisingly and deplorably neglected up till that time.

In 1945 he resigned the position of Professor of Biochemistry at University College, London, on appointment as Director of Research to Boots Pure Drug Company, Nottingham, but until 1946 he was seconded to the Ministry of Food to continue his work there.

In 1944 he was knighted for his war services, and in the same year he became a Fellow of the Royal Society. The latter distinction, having been delayed until late in life, brought more satisfaction to his many friends than it did to Drummond himself and he took little interest in the affairs of the Society. Abroad his contributions during the war were widely recognized. He received the U.S. Medal of Freedom with Silver Palms, and in 1946 he was elected an Honorary Member of the New York Academy of Sciences. He was made a Commander (Civil Division) of the Order of Orange Nassau for his services to Holland, while the University of Paris conferred on him the degree of Doctor *honoris causa*, an honour which particularly pleased him. Then in 1946, his war-work being complete, he moved to Nottingham.

Last Years 1946-1952

At Nottingham Drummond threw himself into the duties of his new post as Director of Research to Boots Pure Drug Company with the same energy and enthusiasm that had carried him through the difficult and strenuous years of the war. But he now possessed an assured reputation that rendered the assumption of his new life much easier than were his early days at the Ministry of Food in 1939. As in the Ministry, so in industry, he could often get to the heart of a problem by his ability to grasp the situation as a whole and to disregard the fears of those whose absorbed interest in detail did not permit them to comprehend the principles involved. His reputation gave such prestige to the research in which he was interested that funds could usually be found for what was needed, and he brought a steady stream of academic friends and colleagues to give informal lectures on their special interests in research. In this way Drummond hoped to diminish the barriers that inevitably exist between industrial research workers and their academic counterparts.

He still continued to travel frequently in connexion with his duties, both in this country and abroad, and again his power to mix easily with all sorts and conditions of men was of immense value.

His wideness of interest is illustrated by the encouragement he was able to give to veterinary research ; his support and enthusiasm were of great importance in the enlargement and equipping of his company's veterinary science division at Thurgarton. At the same time he had become a Founder Member and first Chairman of the 'Fine Chemicals Group' of the Society of Chemical Industry. Although he still continued to give occasional lectures (179, 180, 181) his writings necessarily appeared less frequently than during his earlier days, and from 1948 until his death nothing serious appears to have been published over his name.

Early in 1952 he suffered a sub-arachnoid haemorrhage, which however, appeared to have no lasting effects. In August 1952 he went to France for a holiday with his wife and ten-year old daughter and on the night of August 4-5, all three of them were murdered by shooting while camping near the village of Lurs in the French Alps. *The Times* of 16 November 1953 reported that Gaston Dominici, a 76 year old farmer of Lurs, had on the previous day confessed to the murder of Sir Jack and Lady Drummond and their daughter in their roadside camp on the boundary of his farm, on the evening of 4 August 1952. According to *The Times* of 7 May 1954 a special court at Aix en Provence had recently decided that Gaston Dominici should stand on trial on a charge of murdering Sir Jack Drummond, his wife, and their daughter, at Lurs in August 1952, but no date was given for the proposed trial. No certain motive for the murder had emerged during the investigations which led up to the trial, though robbery had apparently been excluded.

The news of such a brutal and inexplicable crime against an internationally respected and distinguished scientist was received with horror in all civilized countries and led to the publication of many tributes from abroad (see for example *Nature, Lond.* 1952, **170**, 1131; *Chem. Ind. Rev.*, 1953, p. 74).

In this country a group of Drummond's colleagues formed a Committee under the Chairmanship of Lord Woolton to collect a fund with which to endow a Drummond Research Fellowship in Nutrition (*The Times*, 17 July 1953). As a result the first Drummond Fellow took up his duties in October 1954 (*The Times*, 17 Sept. 1954).

The Man

Drummond's life provides a striking example of the man who was supremely well prepared for a task which he was free and able to undertake at a time when his equipment and experience were probably unique. For this undertaking he was fortunate to enjoy energy and good health to a degree unusual in a man of his age and varied experiences. Although he was 61 years old at the time of his death, he was described as a man of about 40 by a French newspaper reporter who came to the scene of the crime before the identity of the body had been established. Indeed, Drummond acted as though he were a man twenty years younger than his age, particularly in the vitality and energy which he displayed right to the end.

His was a warm, gay and cheerful personality: a man who looked for and took appreciably with both hands the nicer things of life. He has been described by a friend as 'a lovable, amusing, kindly, unpredictable soul. His anxiety to help and to please left him with so much to do that he taxed himself far too much.'

Although he was sprightly, debonair, breezy and approachable he was not one with whom intimacy was easily come by. His humour was often puckish and sometimes tinged with cynicism; he was occasionally outspoken, sometimes to the point of brusqueness and on occasions even to his disadvantage, particularly in the earlier years. He was, in fact, at times a controversial figure and his contributions to the advance of science were sometimes themselves controversial. Indeed, it was more through others and through his influence on events than by his own direct additions to scientific knowledge that Drummond made his greatest contributions.

He had a very definite artistic appreciation and ability, often humorous. He was enthralled by a good theatrical presentation and was a frequent 'first nighter' in London. At Nottingham he was a director of the Playhouse Repertory Theatre and did much to keep it on its feet. Good food and wine appealed to him and he became an early member of the Wine and Food Society: when he moved to Nottingham he became a pillar of the local branch of that society. His interest in the problems of a local restaurateur who sought a licence to sell wine was humorously portrayed in the delightful pen-sketch he wrote for *Wine and Food*, No. 76, 1952 (subsequently reprinted in *Chem. Ind. Rev.*, 1953, p. 33). From this delicious study in the spirit of Clochemerle one can catch at Drummond's civilizing influence in an industrial Midland city, where much more than lovely old buildings had been swept away under the torrent of wealth which poured from the textile mills in the last century, and where the arts and gracious ways of living had been swamped to such an extent that they had not yet completely recovered.

He went through sometimes surprising phases because he attuned himself as fully as he could to the circumstances surrounding his immediate interest. Thus when he went to Spain to lecture he went all Spanish for quite a time : in anticipation of going to Russia and Central Europe in 1936 he went all Russian, took lessons in the language, dined at Russian restaurants, favoured the ballet, Russian music and literature. Some of these phases lasted some time, others were soon displaced. A close friend has described him as 'an artist who had got into the wrong pasture'. His intensity of living, his fullness of life, his sustained vivacity, were to some extent a reaction to a way of life to which chance had brought him. He nearly always worked at home with a background of soft music; he said that things came to him better when tuneful music was being played. Undoubtedly his artistic perception and intuition were both his strength and his weakness in matters which were purely scientific.

After his death there was published a letter, addressed to the late Sir Gilbert Morgan in 1935 (then Director of the Chemical Research Laboratory at Teddington), inquiring about the possibility of obtaining basic and acidic resins with which to investigate their use in the separation of amino-acids (*Chem. Ind. Rev.*, 1953, p. 85). In this Drummond was fifteen years ahead of those who succeeded in such an application of ion-exchange resins but unhappily he appears not to have pursued his inspired idea at the time of its inception.

Artist, discoverer, reformer, historian, host: whichever of his many facets caught the light shone brilliantly. But the torch that he lighted at University College and at the Ministry of Food will, in the hands of his pupils and successors, continue to burn steadily through many years to come.

I am particularly indebted to Dr N. C. Wright for information concerning Drummond's activities at the Ministry of Food during the war, and to Dr J. Vargas Eyre for much valuable material relating to Drummond's life. To Professor G. F. Marrian, Mrs Mabel Drummond, Dr J. F. Ward, Dr Magnus Pyke and to many others too numerous to name I express my thanks for information without which this account could not have been written. Lord Woolton has kindly read the typescript.

F. G. YOUNG.

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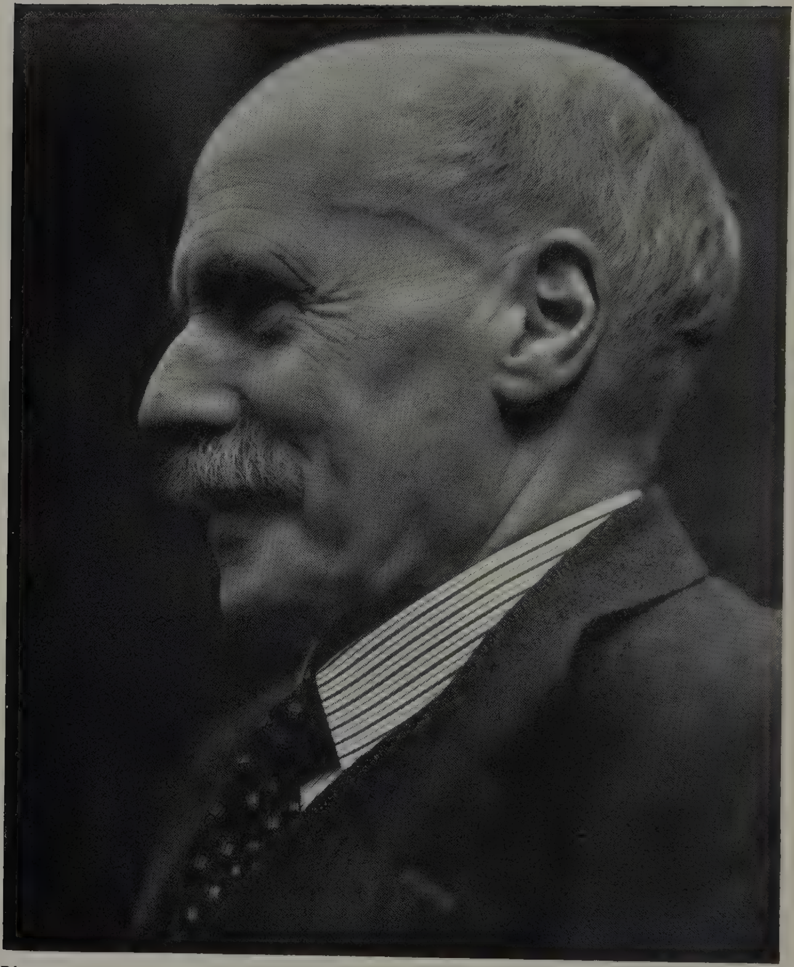

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H. E. Hinton

FELIX EUGENE FRITSCH

1879-1954

Felix Eugene Fritsch was born on 26 April 1879. His father owned a small but very successful private school at Hampstead where his son was born, and Felix's childhood was spent.

Mr Fritsch's gifts were primarily mathematical and musical, though his interests were centred on the latter and on classics. The musical bent was evidently an hereditary one since not only was the grandfather of Felix an operatic singer, but his grandmother also. Even in his latter years it was a pleasure to listen to old Mr Fritsch singing German leider. Felix inherited alike the mathematical capacity of his father and the lack of interest in its pursuit, but the parental delight in music was fully shared. Felix learnt to play the violin, and although he never attained great executive ability, he would justly describe himself as 'a useful member of a quartet'.

In later years the musical week-ends which Felix Fritsch and his wife organized bore witness to their mutual interest in chamber music. These were delightful informal occasions when classical quartets and quintets were the fare provided during the afternoons of Saturday and Sunday. Felix usually participated in these performances, which were often of a high order of excellence, and sometimes Gustave Holtz would be present amongst the appreciative audience.

The school life of Fritsch was marred by recurrent illness, and at the age of sixteen the doctor told his father that Felix would never be strong enough to earn his own living, so that what he studied or did need not be dictated by consideration of a career. His mother had died from lung trouble, hence when the son developed asthma, the prognosis for his future was understandably pessimistic. In fact it was his father who chose botany as a career for Felix, under the naïve belief that this would mean a largely outdoor occupation which might improve the boy's health.

At this period Felix had passed the London Intermediate Examination, and he realized that, at home, he was treated so much as a tender plant as to have but little prospect of the growth and development that even his physique then permitted. With the quiet pertinacity that was so characteristic of him in later years he freed himself of these trammels and obtained a post at Munich as Assistant to Radlekofer, whose interests were centred in systematic anatomy, an accident that may well have played no small part in determining subsequent events.

On returning to England in 1902, Fritsch obtained a part-time post at University College, London, as an Assistant in the Botanical Department to Professor F. W. Oliver, F.R.S. The latter was of course intimately associated with Kew, since his father, Daniel Oliver, had only retired from the Keepership of the Herbarium twelve years previously, and the then Director, Sir William Thistleton-Dyer, and Dr D. H. Scott, who had been Honorary Keeper of the Jodrell Laboratory, were his close friends. When, therefore, assistance was required in the Jodrell, Oliver at once recommended Fritsch as possessing the requisite qualifications. Prior to Hitler's war, the number of State Schools which provided adequate instruction in the physical sciences was very limited, so that student gardeners were accepted at the Royal Botanic Gardens who had not received any formal training in chemistry or physics, and courses in these subjects were then given to them at Kew. In March 1903, Fritsch was appointed to the post of Lecturer in Physics and Chemistry at Kew, a position which he held for the following six years. It was not merely that he knew sufficient of these subjects that led to his selection, but doubtless mainly his suitability for another role owing to his proficiency in German, and even more his training under the anatomist Radlekofer.

In 1908 Hans Solereder had produced his classical work on the *Systematische Anatomie der Dicotyledonen*, which was one of the great botanical works that at the instance of Sir Isaac Bailey Balfour, were being translated for the Clarendon Press. L. A. Boodle, the Keeper of the Jodrel Laboratory, who had become a recognized authority on taxonomic anatomy, was entrusted with this task, and it was to assist him that Fritsch's help was enlisted and for which he was so suitably equipped.

Whilst in Germany Fritsch had become interested in the publications which had recently appeared of Bruno Schroder and Zacharias on river plankton, so that in the summer vacation of 1902 he collected samples from the Thames, between Kew and Cookham, and published a note on these in the *Annals of Botany* in September of that year, in which he listed fifty-nine organisms. It is of interest to recall that in this paper we see the beginnings of the conception, which he was later to bring to fulfilment, of a Freshwater Biological Institute, in his comment that we possess nothing whatever comparable to the inland biological stations established on the continent. In the same year appeared a paper on the juvenile state of *Oedogonium*, as from the Jodrell Laboratory, in which we note that he expresses his indebtedness to L. A. Boodle, showing that he was already working there prior to his appointment as a lecturer to the Student Gardeners. In January of the following year a note appeared in the *Annals of Botany* on Kew algae, in which he called attention to the seasonal changes in the algal flora, and this may be regarded as the inception of his periodicity studies which were to be the major contribution of his investigations for some years. There was, however, one group of organisms that particularly attracted Fritsch's interest at this time, namely, the Cyanophycae (Myxophyceae), a group fascinating alike from the peculiarities of their cellular organization, the problem posed by the heterocysts and their remarkable capacity for endurance of extremes of temperatures and moisture.

We find him describing a new species of fungus parasitic on *Tolypothrix* which was collected by Boodle, from which we may infer that the latter had encouraged this interest. Studies on these algae appeared in various periodicals, and Fritsch returned to their consideration in later years.

At University College Fritsch remained as an Assistant for four years and as an Assistant Professor from 1906-11. The art of lecturing is no easy task since it involves the transfer of a pattern of logical thought from the mind of the speaker to those of his hearers, to achieve which the choice of phraseology and the apt word even the tone of voice, can alike assist materially in rendering one's own mental edifice manifest to one's audience. Just as a good drawing depends in no small degree upon economy of line so too the good lecture depends on a discriminating choice of carefully selected data, the wider background being provided by the text-books.

When Fritsch began giving advanced lectures on the algae in the first decade of the century almost the only text-book available was Oltmann's classical *Morphologie und biologie der Algen* which appeared in 1904-5. West's *British freshwater algae* published in 1902 was essentially taxonomic and indeed no comprehensive work in English on the phylum was available until Fritsch himself filled the gap, by two volumes notable for their erudition, in 1935 and 1945. Thus anyone lecturing on the subject in the early part of the century had perforce to provide information at the expense of the time available for developing the concepts that gave coherence to the morphological and biological data. Perhaps for this reason, though Fritsch as a lecturer was both methodical and thorough, he suffered from the defects of his virtues remaining rather prone to overload his discourse with facts, a trait that tended to conceal their fundamental worth. As a demonstrator, however, he excelled in his painstaking and cheerful expositions to overcome the difficulties of students and illumine what was obscure.

Mechanisms, as such, had no interest for Fritsch, and it would be true to say that he tolerated them for their utility and found little, if any, pleasure in their use. As a consequence he was never particularly skilful with his hands and relied on the assistance of others for the illustrations to his papers, although in his later years he, with his usual pertinacity, acquired a measure of skill in delineation. His dislike of mechanisms was never more manifest than when the small glasshouse at University College in which he grew his earliest algal cultures required heating with an oil stove. This he tended, until one day the Department was enveloped in a pall of soot, whereupon he ruefully resigned the care of the stove for the future to other hands. His cheerful acceptance and recognition of his limitations was one of the attractive aspects of his personality and rendered him a good companion in the field where otherwise his asthmatic disabilities might have been irksome.

The first of the 'Studies on the occurrence and reproduction of British freshwater algae in nature' appeared in 1907, and this paper, dealing with *Spirogyra* was written in collabortion with Miss Florence Rich, an association that was to be fruitful over many years. There followed a study by the same authors of monthly observations over a five-year period of the algal flora of a pool near Bristol, and of a series of fortnightly samples taken by the present writer over a period of four years from a pond in Hertfordshire. Both showed a similar periodic trend despite marked dissimilarity in the constituent species. From this time onwards Fritsch's activities became less ecological and more taxonomic. Already in 1906 Fritsch had published an account of the freshwater and sub-aerial algae of Ceylon, but the change of emphasis in his approach after the first decade of the century was due in large measure to the fact that for the next twenty years he became almost wholly engaged in the examination of collections made by others. Thus in 1911 were described algae from the S. Orkneys, in 1912 an account of the *Discovery* collections from the Antarctic, in 1914 descriptions of the collections of P. A. Methuen from Madagascar.

The first report on 'The freshwater algae of Africa' (1918) was based upon samples collected by various botanists. It recorded representatives of ninety-seven genera and 238 species including nineteen which were new discoveries. The most interesting of these latter were two additions to Bohlin's genus of so-called dendroid Chlorodendraceae, *Ecballocystis*; a remarkable *Scenedesmus* forming large plate-like colonies; a *Ulothrix* with thick gelatinous walls and an unusual chloroplast; also a distinctive *Schizothrix* with as many as fifty trichomes twisted within the sheath; but most remarkable of all was a species of *Sphaeroplea* in which the septas were produced by ingrowths that possibly failed to meet completely in the centre and with a chloroplast that had an ill-defined reticulate appearance. This rich harvest led to further studies in 1921, 1924 and 1929. The second contribution dealt with samples from the Transkei Territories, mostly collected by Miss Pegler, and yielded 146 species, exclusive of diatoms, of which six were new, mostly desmids and a richly branched *Enteromorpha*. The third paper described material collected by the late Professor Bews from Natal. These provided a total of 228 species chiefly Desmidiaceae, Myxophyceae and diatoms. Amongst them fourteen species were new, of which one was placed in a new genus of Ulotrichaceae characterized by a chloroplast in the form of a complete peripheral ring. The blue-green algae were a notable feature of most of the Natal samples, particularly as primary colonizers, and furnished several new species of which the richly branched *Scytonema splendens* was perhaps the most distinctive. Fritsch saw in such Myxophyceae further support for his views on the heterotrichous habit though others might regard them rather as evidence for the exploitation of stratified environmental conditions imposed by natural selection on diverse types of organisms as parallel expressions of survival value.

In the final paper dealing with African algae Fritsch and Miss Rich described collections, mostly made by Miss Wilmern from Griqualand West, which furnished another 182 species, sixteen of them new, and including one free-floating member of the Rivulariaceae for which another genus was created. Thus we see that these studies from the African continent not only added fifty-six species and a number of varieties but had very considerably extended Fritsch's knowledge of the world's freshwater algae. Taken together with his earlier studies they collectively comprised so wide a range geographically and climatically as to establish Fritsch's reputation as a leading authority on the taxonomy of the freshwater algae. Moreover, the second edition of West's *Treatise of the British freshwater algae* which he produced in 1927, demonstrated that the mantle of West had fallen upon him and his two scholarly volumes on the *Structure and reproduction of the algae*, constitute an enduring monument to a lifetime's achievement.

Fritsch's name is appropriately commemorated by a terrestrial member of the *Chaetophoraceae* from Mysore, described by Professor Iyengar under the name *Fritschiella tuberosa*. The terrestrial algae had a special attraction for Fritsch, as is manifest from the papers he wrote on their moisture relations in 1922 and 1923, a recrudescence of the interest already displayed in his lecture to the Geographical Society on the role of algae as pioneers on new ground, and it was again the topic which he chose as his contribution to the Memorial Volume to Professor Setchell in 1936.

The capacity of the algae that live on the soil surface to survive rapid and extreme dessication with but little alteration in their vegetative state was a feature to which Fritsch drew attention and his measurements showed that under drought conditions these organisms soon attained a relatively steady minimal state, although any increased humidity of the atmosphere rapidly led to an augmented water content of the algae. The fact that apart from such outstanding sub-aerial green algae as *Pleurococcus*, *Cystococcus* and *Stichococcus*, the majority of the characteristic terrestrial types are members of the Myxophyceae (Cyanophyceae), no doubt enhanced the interest for Fritsch of this algal community. Their astonishing capacity for recovery from plasmolysis rendered these terrestrial types peculiarly suited to the role of primary colonizers on virgin soils and rock surfaces, where Fritsch demonstrated the succession from unicellular to filamentous types, followed by mosses or the synthesis of lichens, to be of widespread occurrence. These studies of the sub-aerial and soil algae led to the recognition by Fritsch of many new species and some new genera. Thus in 1942 he described *Chrooderma endophytica* as a new genus and species of Trentepohliaceae found growing as an epiphyte on dead bramble stems in the west of England. Two years later he described a new terrestrial genus of the Cladophorales growing on limestone and in the paper on soil algae of 1942 he listed over eighty species of which thirteen had not previously been described.

In his latter years evolutionary trends came to occupy more of Fritsch's thoughts and writings. He found in the parallel development of a somatic differentiation into a prostrate and erect region in the various algal groups a compelling argument for a probable starting point towards the evolution of the higher plants. If his speculations in this connexion appear too readily to accept homology where there may be but analogy and to create the illusion of a plausible phylogenetic sequence without sufficient regard to the weaknesses of the superstructure, nevertheless they serve as the basis for an integrated treatment of the facts of comparative morphology the value of which is undeniable. Evolutionary trends were the subject of two of his Presidential Addresses to the Linnean Society but in a third he returned once more to the blue-green algae, discussing in an admirable survey the views concerning the Heterocyst, 'an enigma' as he called it, which had always intrigued him.

When Fritsch went to East London College (now Queen Mary College) in 1911, he found a department ill-equipped and ill-staffed. He built up both with steady perseverance. In 1913 the present writer was appointed as Lecturer under Fritsch, and I can bear testimony to the difficulties we had to overcome, which, though an excellent experience for a young man, were arduous to one who not infrequently spent the night in a chair suffering the spasms of an asthmatic attack.

Having been invited, at this period, by Messrs Bell & Sons, to write an elementary text-book of botany, I agreed on condition that I might invoke the collaboration of someone who would supplement my own, then meagre, experience of examination requirements. This was the beginning of a successful partnership that extended to five volumes and various editions, and lasted almost to the final years of Fritsch's life, since we prepared together a new edition of *Plant form and function* in 1952. Fritsch was quite a competent typist so in 1914, when *An introduction to the study of plants* was produced, after discussing together the contents of a chapter, Fritsch typed as I extemporized the text, and so absorbed were we in our respective tasks that the maroons and air raids would pass unnoticed. The room in which we worked was a flimsy structure on the top floor, so it was suggested we were attempting to obtain an unfair start on the journey to a better world.

At Queen Mary College Fritsch played a notable part not only as head of the Botanical Department over a period of thirty-seven years, but as the first Dean of the College Faculty of Science and he was appropriately admitted to the Fellowship of the College in 1948. He had a remarkable capacity for dealing with administrative matters in which his orderly mind was a great asset.

To the affairs of Queen Mary College and London University Fritsch gave whole-hearted devotion. He was elected a representative of the Science Faculty of the Senate in 1944 and became chairman of two of its committees, both of which involved onerous responsibility and for which his methodical approach was well fitted. In 1946 he was appointed chairman of the committee dealing with recommendations for research grants from the Central Research Fund for investigations in the fields of engineering, medicine and science. Every year this body assesses more than a hundred applications and concerning the relative merits of these Fritsch assiduously enquired, indeed not long before his death he provided notes on the applications to be considered at a meeting of this committee that he was too ill to attend. In 1949 Fritsch became Chairman of the Scholarships Committee, the work of which is very heavy and to this too he devoted the same painstaking consideration.

In the Presidential Address which Fritsch delivered to the Botany Section of the British Association in 1927, he returned once more to the theme that there was no biological station in Britain for the study of freshwater problems. This led to a subsequent discussion on the topic, and to the founding of the Freshwater Biological Association in 1929, with Professor Fritsch as Chairman of its Council—a position which he still retained at the time of his death. Not only was the existence of this body a direct outcome of his initiative, but it owes a great debt to his unremitting efforts on its behalf, especially in the first decade of its activities.

The short, stocky figure that Fritsch presented, with his genial expression and equable, affectionate temperament, was a feature of most botanical gatherings of the past forty years, where he won the regard and esteem of all his colleagues.

Apart from music, it is probably true to say that the more artistic and poetic aspects of life left him almost untouched, either as a stimulus or a distraction. His achievements in the face of ill health were the outcome of sheer industry combined with exceptionally methodical habits. The card index was one of his most cherished tools, and it is symptomatic that he derived such manifest pleasure from the creation of what came to be known as his 'scrap collection', which was, in fact, an assemblage in the manner of a card catalogue, of all the illustrations of algal species from original papers or other sources that he could obtain.

For his scientific contributions to algology, Fritsch was elected to the Royal Society in 1932, and was awarded the Darwin Medal in 1950. He was an honorary member of a number of foreign scientific bodies, and was posthumously awarded the Gold Medal of the Linnean Society of London. In 1952 the University of London conferred on him the degree of LL.D. *honoris causa*, and in the following year he was elected President of the International Association of Theoretical and Applied Limnology.

He married in 1905 Hedwig Laskar, who survives him, and there is one son. During his residence in Cambridge Fritsch's asthma greatly improved but it became obvious in 1953 that his vigour was diminishing and early in the following year it seemed as though the old trouble was gaining ground. Actually, however, cancer of the lung had developed to which he succumbed on 23 May 1954 soon after he had attained his seventy-fifth year. Thus was brought to a close a lifetime of rigorous self-discipline and unremitting labour. For permission to reproduce the excellent photograph of Professor Fritsch the Royal Society is indebted to Mr G. C. Monkhouse, F.R.P.S.

E. J. Salisbury

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T. Goodey

TOM GOODEY

1885-1953

The sudden death of Tom Goodey, while on his way home from a meeting of the Society of Friends on the evening of 7 July 1953, unexpectedly robbed nematology of its foremost exponent and applied biology of one of its most engaging and vital personalities. Science is not the only loser, for his varied talents, wide interests and great energy, had combined to make him prominent in many other activities.

Born at Wellingborough, Northamptonshire, on 28 July 1885, Tom was the ninth and last child of Thomas and Hannah Goodey. His mother (*née* Clayson), the daughter of an agricultural worker, died when he was two; his early childhood was largely supervised by his oldest sister and his boyhood by his step-mother. His father, like his grandfather, was a leather worker, who had started work in a skin yard at the age of seven; what his father lacked in formal schooling, he more than made up in initiative and industry, for he rose first to be a skilled machine operator and then to run a small boot factory of his own. For one in his circumstances, Thomas Goodey was an unusual man, and young Tom obviously learnt much from his father that coloured his whole life: an ability to work hard and a respect for those who do so, skill in the use of tools, an interest in natural history and gardening, and a love of music. None of the rest of the family had a voice of the quality of Tom's but they were all competent singers and fond of music. As soon as they were old enough, all of them joined the choir of the Congregational Chapel at Wellingborough and, at home, many evenings were happily spent in singing glees, part-songs and oratorios.

After 11 years at the Victoria Board School, Wellingborough, young Goodey won a county scholarship in 1899 and went to the Northampton Grammar School. He left in 1904 to become a pupil teacher at Irthlingborough Council School, an experience he did not particularly enjoy. In the same year, he passed the Matriculation examination of London University, which gained him admission to the Teacher's Training College of Birmingham University. This also allowed him to start on a course of study for the B.Sc. degree, and, having passed the intermediate examination, he could forget the teacher's training course and work exclusively on zoology and botany, in which subjects he obtained an honours degree in 1908. In his final examination he was bracketed top with two other students; they were awarded two scholarships of £50 each, which they shared between them, and Goodey was launched on the first year of his research career with an income of £33 6s. 8d.

Although this was the beginning of his official research, he had, while still an undergraduate, demonstrated his powers of observation by noticing a previously unfigured groove leading to the gastric pouches of the jellyfish, *Aurelia aurita*. Both his professor, T. W. Bridge, F.R.S., and lecturer in zoology, Leonard Doncaster, F.R.S., were favourably impressed by his industry and ability, and they encouraged him to extend his studies of this organism; with their help, he worked out the structure and function of this groove, and published his results in two papers, one of which appeared in the same year as he graduated. Continuing his post-graduate work under Bridge, he divided his time between studying the anatomy of the frilled shark, the catfish and dogfish.

In 1909 he took his M.Sc. degree and was awarded a further research scholarship of £50 for one year. At the suggestion of Professor Gamble, F.R.S., who had succeeded to the chair of zoology, after Bridge's death, Goodey moved to Rothamsted Experimental Station early in 1910. This entailed a considerable change in his line of work and a move into a subject full of controversy. Russell and Hutchinson had announced that soil contained protozoa that prey on bacteria. The announcement had aroused interest because of its bearing on soil fertility, and opposition because it conflicted with the general view that the only active organisms in soil were bacteria. Several papers were published by workers who stated either that the soil contained no protozoa or that, if there were any, they occurred only as cysts. Gamble was interested by the problem and, when Russell approached him to suggest someone to work on it, he recommended Goodey as his best student.

Goodey started his work at Rothamsted while still holding the scholarship from Birmingham University, but was soon awarded the Mackinnon Studentship of the Royal Society. Comparatively, at £150 a year, this seemed wealth, and on 27 July 1912, he married Constance Lewis, whom he had met while they were both students at Birmingham.

Hutchinson's culture technique had mainly isolated *Colpoda cucullus* from soil and, on the assumption that this was the chief protozoan there, Goodey was set the problem to see whether it was active or only encysted. There was no one who could offer him any useful advice on techniques, and so his first task was to devise them. Having succeeded in this, he showed conclusively that *C. cucullus* was normally encysted and inactive in soil, and his results also led him to doubt whether any soil protozoa functioned as predators that limited bacterial populations. When his work was described it was so convincing that most people were satisfied that the predator theory was finished, but Gamble, realizing that Goodey had been able to study only the ciliates, suggested there was still the possibility that amoebae were active in soil. This idea was later found to have substance, but unfortunately Goodey could not pursue it at the time, for his scholarship had expired and other money obtained for the work at Rothamsted had also been used.

He returned to Birmingham in 1913 to a post in Gamble's laboratory, where he continued to work on the identification and biology of protozoa, mainly from soil, but he also collaborated with A. W. Wellings to show that an amoeba inhabiting the human mouth was not a harmful parasite as previously thought, but a useful scavenger that feeds on bacteria and on the remains of salivary corpuscles. During the first world war he also acted as protozoologist to the 2nd Southern General Hospital at Birmingham and made the routine examinations for *Entamoeba* sp. and other intestinal protozoa.

From his childhood onwards, Goodey had loved singing and as an adult he developed into a first-class performer, first as a baritone and later as a true tenor. While living at Harpenden he studied voice production in London during 1910-11 and he continued his training at Birmingham. At first he sang solely for the pleasure it gave to him and his friends, but as his reputation and family grew (a son in 1914, twin daughters in 1916, and two more daughters, one in 1919 and the other in 1921), in addition to singing in choirs, amateur musical and operatic societies, he also engaged in professional singing, starting as the paid tenor soloist in a performance of 'The Messiah' at Dudley in December 1916. The musical critics gave him highly favourable notices. Ernest Newman, for instance, wrote in 1918: 'Dr Goodey's curiously fine phrasing, that is always so sensitive to the melodic and rhythmical contours of the music, made his singing . . . a constant musical pleasure'; or again, also from the *Birmingham Post*: 'As always his phrasing was a live salient thing, and his diction as free from reproach as the intelligence of his singing . . . He is a notable example of the truth that in serving music first and foremost an artist may best serve himself.' It would be a fair interpretation of Goodey's character to say that the word music in the last sentence could equally well be replaced by any of his other interests and remain equally true. As in his music he never played to the gallery to gain applause by departing from his interpretation of what the composer intended, so in his science and other activities he never allowed his own immediate interests to change his decisions from what he knew to be right and proper. His high technical skills were matched by a keen intelligence and shrewd judgement, but his most outstanding qualities were absolute reliability and a fearless honesty.

When in 1920 he returned to Harpenden, where he lived for the rest of his life, Goodey's reputation as a singer was already made, and demands for his performances steadily increased. An invitation to take part in Rutland Boughton's 'Bethlehem' led to an association with Boughton's music that extended over many years. After playing the part of Midir in 'The Immortal Hour' when it was produced in Birmingham, he was understudy for it in London during 1922, but as the principal was often absent Goodey played the part there many times. The part of Angus in Boughton's 'The Ever Young', a sequel to 'The Immortal Hour', was written mainly for him, and he sang it at Bath in 1935. Goodey was perfectly at home on the stage and, in addition to his superb voice, he was an accomplished actor. Although he considered the opinions of those who advised him to take to the profession of singing as being more financially rewarding than biology, he decided to make science his work and keep his singing as a recreation and a means of occasionally supplementing his income. Singing never seemed to tire him, and his evening and week-end activities did not stop his work in the laboratory. However, the publicity he received from his performances, not only in the theatre and in the concert hall, where he specialized in recitals of lieder by Schubert and Hugo Wolf and in English songs, but also from his frequent broadcasts from 1925 onwards, became embarrassing to him as a scientist and to his professor, and in 1927 he adopted the stage name of Roger Clayson.

Although Goodey had many memorable public successes between 1920 and 1940, he will probably be remembered most vividly by those who were privileged to hear him on more intimate occasions, when the pleasure came not only from the beauty of his singing but also from his own evident enjoyment. Although, as with everything else he did, he took his singing seriously, he did not take it solemnly. And, with all his public successes and his meeting on equal terms with the principal singers of the time, he was always ready to appear with the wholly amateur in homely and modest surroundings. He was willing, too, to mix fooling with his fine singing, and this to the end of his life. His colleagues at Rothamsted will never forget how, at the age of 65 or more, he was always the most youthful performer at the Christmas Parties, frolicking and singing in a way that made his fellow performers, some with less than half of his years, look as though they had never really known what it felt like to be young.

His opportunity to take such an active part in the musical life of London came from the chance that his scientific work was also being done there. It was typical of his great energy that, while he was filling all these engagements, he was also busily developing a new subject. The staff of Rothamsted Experimental Station was enlarged at the end of the first world war, and Goodey was offered and accepted an appointment there in 1920. He then started his work on helminths, work that was to occupy him for the next 33 years, beginning with a study of the clover stem eelworm. This spell as a plant helminthologist, however, was short, for in 1921 he joined Professor Leiper's department at the London School of Tropical Medicine, and for the next five years he worked mainly on the parasites of vertebrates. During this period he developed his 'floating raft' technique, whereby the manner in which infective larvae penetrate skin could be observed directly under the microscope. He did many other things, but this and his morphological survey of species of *Oesophagostomum*, exemplify the two types of work that gave him the greatest satisfaction, studying the behaviour and habits of living eelworms and finding the morphological differences between a related group of species.

With the development of the Institute of Agricultural Parasitology at St Albans, Goodey became the senior member of the staff, and he remained in this post until, on Leiper's retirement, the Institute closed in 1947. From 1926 on he specialized in studying plant-parasitic and free-living eelworms, the subject matter of what has come to be called nematology and in which he soon became acknowledged as a world authority. At that time all the important nematodes affecting plants were grouped into three large genera, *Tylenchus*, *Aphelenchus* and *Heterodera*. Others in the Institute were studying *Heterodera* spp., so Goodey tackled the other two and wrote several papers on the comparative morphology of *Aphelenchus* spp. and many on the species of *Tylenchus* (called *Anguillulina* after 1930). He did not neglect the economically important stem eelworm, *A. dipsaci*, but he was attracted more by the gall-formers, which have the added interest for a natural historian of producing variable responses in their host plants. These never failed to fascinate him and in his last year he started to study them anew.

In 1932 he produced a taxonomic monograph describing 60 species of the genus *Anguillulina*. Like nearly all his work, this was published in the *Journal of Helminthology*, a journal in which his name appears in every volume, from the first in 1923 to 1949, with an average of more than three papers in each. These reveal, not only his immense productivity, but the patient and meticulously careful work of the good taxonomist, which is what Goodey pre-eminently was, even though much of his work is outside the scope of usual taxonomy. The fall in publishing to three papers in the five years after 1948 does not reflect any slackening of his industry or interests, but his increasing responsibilities in setting up and administering his own department, and the writing of his second book.

The large monograph was largely preparatory to his first textbook, *Plant parasitic nematodes and the diseases they cause*, which was published in 1933. This, based primarily on his own work, described the morphology and life-history of each important parasite, together with the diseases they cause and the methods whereby they can be combated. It was a notable landmark in the development of nematology and it has stood as the standard work, although long out of print and considerably out of date. With his son Basil, he was busily engaged in preparing a new edition when he died.

Goodey was an observer rather than an experimenter. He combined a perceptive and critical eye with a large fund of patience, an invaluable endowment for work in taxonomy and bionomics. His great observational powers and his pleasure at unravelling the complexities of natural phenomena perhaps show best in a piece of work that is remote from taxonomy. While studying tulip root of oats, he discovered a nematode parasitic in the frit fly, itself a parasite on oats. His unravelling of the inter-relationships between eelworm, fly and plant, are well described in an outstanding paper published in the *Philosophical Transactions of the Royal Society* for 1930. His powers of observation and his practical turn of mind also show in two of his other discoveries; one, that some varieties of winter oats resist attack by the eelworm that causes tulip root, and two, that the eelworm causing bloat of onions can be carried by seed, which, however, can be readily disinfested by fumigation with methyl bromide.

As a taxonomist Goodey was conservative, a 'lumper' rather than a 'splitter'. Consequently, although he recognized that the many species of *Tylenchus* were not all congeneric, he left it to others to distribute them among several genera. This was the only thing in which he was conservative. In techniques he was always looking for improvements. For instance, although his own standards of microscopy were high, as is evident from the excellent camera-lucida drawings that illustrate most of his papers, when, at the end of the second world war, he found that American workers had better techniques, he was immediately eager to follow them. His interest in techniques led him to produce in 1949 a bulletin entitled *Laboratory methods*, which was so successful that a new and enlarged edition was needed within two years.

From 1922, in addition to working constantly with plant parasites, Goodey also studied free-living nematodes, and his second textbook in 1951, *Soil and freshwater nematodes*, described the morphology, biology, food habits and predatory behaviour of 190 genera. When this book appeared, he had already been back at Rothamsted for three years of his third period there. The Institute of Agricultural Parasitology ended in 1947, and the members of its staff studying plant nematodes became the Department of Nematology of Rothamsted Experimental Station, with Goodey as its head, and they moved to new laboratories there in 1948. These were years of great activity, and a period when he had the satisfaction of seeing that the quality of his scientific work was being recognized. In 1947 he was elected to the Royal Society, and he toured the United States of America. In 1948 he took over new laboratories built specifically for his subject. In 1949 he gave a training course in nematology at Cambridge, and in 1951 another at Harpenden, this time international and sponsored by the Food and Agricultural Organization of the United Nations Organization. In the New Year's Honours List for 1950 he was awarded the O.B.E., and in 1951 he visited several European laboratories where nematology is studied. He had accepted an invitation to visit Australia in 1953, to advise the government there on nematological research, and had almost made his preparations when he died. Perhaps all this activity hastened his end, but he showed no signs of being overburdened; cheerful, serene and sprightly, he seemed tireless and capable of anything. Indeed, in addition to all his other activities, on changing houses at the age of 65, he again demonstrated his love of gardening by constructing yet another beautiful rock garden. His youthfulness was such that official retiring ages were obviously not meant for him, and he remained as head of the department for another two years. After retiring from this post, his work did not abate, for he took his freedom from administration simply as an opportunity to devote more time happily watching his nematodes.

Goodey had long thought that the universities sadly neglected nematology and that at least one should have a post-graduate school in the subject. As early as 1936 he devoted a considerable part of his presidential address to the Association of Applied Biologists to a plea that the subject should be taught, but the plea went unanswered until he himself organized two courses. It was obvious to those who attended them that some university had missed a great opportunity by not setting up a school with him to lead it. Despite his early dislike of pupil teaching, he was an excellent teacher. He lectured lucidly and with such an evident joy in his subject that his enthusiasm must surely have infected students as thoroughly as it did his fellow workers.

Goodey's ability to speak with power and lucidity also contributed to the prominence he gained in the Society of Friends. He was a deeply religious man in the sense that he recognized a higher power entitled to his reverence and obedience. Scrupulous in all his dealings, he knew there were absolute standards of truth, honesty and fidelity, which must never be departed from for worldly gain. He loved to sing church music, but his religion needed no ritual. Brought up, like the rest of his family, as a member of the Congregational Church, in 1933, a few months after his wife had done so, Goodey also joined the Society of Friends. In their beliefs and in the simple serenity of their meetings, he found a completely satisfying spiritual home. He served as Clerk of the Bedfordshire Quarterly Meeting from 1942 to 1946 and was an Elder at the time of his death.

The fact that he had high ideals and set strict standards did not make him in any way a prig or a prude. He had an unrestrainable sense of fun, was always lively and enjoyed both telling and listening to a good story. Good humoured and tolerant, with a wide range of experiences, he was excellent company. Normally patient, he would suffer fools only as long as they were young and well-meaning. When he detected pretentiousness, sham or self-seeking, then his righteous indignation would show in the plainest of speaking. This, too, could show in his writing, and any doubts about his ability to engage in polemics will be dispelled by reading his paper (1924) on hog lung-worms.

The sudden quenching of Goodey's immense vitality was a severe shock and left his many friends with a deep sense of personal loss. It is certain, though, that he would not have had them mourn, and he would possibly have approved his departure before age had touched his enthusiasm or dimmed his perception. Certain it is, too, that his affairs were in order and that he was fully prepared, even though his friends were not. While knowing only too well that the world is the poorer for his death, it is more in keeping with his outlook that we should dwell on the fact that it is also much richer for his life, an exemplary life, full of the richness that comes to a talented man who devotes his talents to increasing the sum of human knowledge and happiness.

In preparing this notice, I have received much information and help from Mrs Tom Goodey, Sir John Russell, F.R.S., and Dr B. G. Peters.

F. C. Bawden

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M. H. Gordon

MERVYN HENRY GORDON

1872-1953

MERVYN HENRY GORDON, who died on 26 July 1953 at his home at Molesey, was a medical bacteriologist whose whole career was largely devoted to research: he occupied a leading position in this branch of medical science in London for the greater part of the first half of this century.

Gordon was born on 22 June 1872, the sixth of ten children of Canon H. D. Gordon, Vicar of Harting, Sussex. His mother was a daughter of William Buckland, F.R.S., the first Professor of Geology at Oxford and later Dean of Westminster, and was acquainted with many of the leading scientists of her time. The Gordon family was descended from Scots who migrated to the Gower peninsula in the time of Henry VII, and therefore had a strong Welsh as well as Scottish element in their ancestry. His paternal grandfather as well as his father was a Canon of the Church of England, and it was apparently with some hope that he might also enter the Church that Gordon was sent to Keble College, Oxford, after his earlier education at the Dragon School, Oxford and Marlborough. This was done in spite of a verdict from Marlborough that he was 'backward', and a recommendation that he be sent to the colonies.

The father evidently knew his son better than the master who reached this conclusion. It is perhaps significant that no branch of science was taught at Marlborough in those days, and the fact—attested by a contemporary—that Gordon was 'no good at work' at school may well have been attributable to his lack of interest in the restricted curriculum. When he began the study of biological sciences at Oxford he evidently found work more congenial: his first paper, with M. S. Pembrey, is evidently based on observations made while he was at the university, and he took his B.A. degree with Second Class Honours in Physiology in 1894. Thence he went to St Bartholomew's Hospital, qualifying with the B.M. degree at Oxford in 1898. He subsequently obtained the Oxford degrees of B.Sc. in 1901 and D.M. in 1903.

Immediately after qualifying in 1898 Gordon began work in the Pathology Department of St Bartholomew's Hospital under Dr Emmanuel Klein. It seems likely that this had long been intended, and indeed that his choice of St Bartholomew's as a London school may have been made with such work in view, since Klein and his family had been frequent visitors to Harting and the Gordons knew them well. His first paper on a bacteriological subject indeed proves by its date that he must have been working in this department before qualification. Whatever other duties he may have undertaken at this time, he was evidently commissioned at an early stage by the Local Government Board to undertake various pieces of research, and within a surprisingly short period must have attained high repute: evidence for this is to be found in the nature of some of his early publications, and particularly in the fact that he was chosen to conduct the following quite exceptional piece of work.

Studies of air hygiene

An epidemic of influenza in the House of Commons and complaints by Members that the air of the Chamber 'lacked freshness' and caused 'a lassitude and feeling of heaviness' led to the appointment of a Select Committee to inquire into the ventilation. This Committee recommended 'the institution of inquiry into problems of ventilation still unsolved, with a view of introducing further improvements into the present system', and the Office of Works appointed Gordon to conduct the inquiry with facilities for pursuing it at Westminster. With the collaboration of a physicist, a chemist, and the Resident Engineer, he conducted a long series of most elaborate experiments in the Debating Chamber itself, for the most part when it was empty, but some even when the House was sitting. This work was done during 1904-5: Gordon's report with recommendations is a document of only 12 pages, but the Appendix with a full account of his work is a Blue Book of 212 foolscap pages with numerous illustrations, and is proof of immense industry, ingenuity and enterprise. He applied to this problem the work of Flügge, who had shown that particles of secretion are expelled from the mouth not only by coughing and sneezing but by speaking, and he relates how a speaker (evidently himself) having filled his mouth and throat with an indicator organism (*Chromo. prodigiosum*) addressed a house empty but for an audience of agar plates for one hour. 'The words spoken . . . consisted of recitations, in a loud voice, from Shakespeare's plays, "King Henry V" and "Julius Caesar".' When similar culture plates were exposed during a sitting, the salivary streptococci of members served as indicators of aerial bacterial spread.

These experiments, and smoke tests, showed that the distribution of air delivered by the ventilating ducts was far from uniform, and confirmed Flügge's finding that oral bacteria could be recovered at considerable distances from their source. It was also shown by the use of indicator bacteria that material was carried from a distant spot in Palace Yard to the matting in the Centre Gangway: 'In short, particulate pollution to which the air of the Debating Chamber is subjected is supplied chiefly by Members themselves, and is either brought in on their boots, or is derived from their mouths and upper respiratory passages.' These are only examples of the findings in a report which is not only most comprehensive, but admirably and in places entertainingly written. When it is said that 'faecal matter' is imparted to the air of the Chamber by members' boots, a footnote qualifies this as 'Derived, almost certainly, from the horse'.

The differentiation of streptococci and other cocci

At about this time Gordon was also engaged, with F. W. Andrewes and T. J. (now Lord) Horder in a study of the characters of streptococci with a view to their more orderly classification. Strains from a variety of sources were submitted to a series of biochemical tests, and capacity to ferment mannite and raffinose and to reduce neutral red anaerobically were identified as characters serving to distinguish three species, *Str. pyogenes*, *Str. salivarius*, and *Str. faecalis*. This classification is now universally accepted, although the salivary species is generally known as *Str. viridans*, and other criteria of identity have come to be regarded as more helpful. A polemic ensued with E. W. Ainley Walker, who contended that fermentative properties in streptococci are too variable to form a basis of classification. This work had an important connecting link with the studies of air hygiene; the presence of salivary streptococci in the air was identified as an index of pollution from the mouth and throat, and has since been extensively used as a measure of such pollution.

Gordon was also interested from his earliest days in the characters and possible further differentiation of one of these species, *Str. pyogenes*, or the haemolytic streptococcus. Two of his publications in the *Reports of the Local Government Board* before the turn of the century were on the bacteriology of scarlet fever, and this subject recurs at intervals, culminating in his serological study of strains from septic infections generally and from scarlet fever, published in 1921. His use of the method of agglutinin absorption and his identification thereby of three serological types may be regarded as the starting point of work continued by F. W. Andrewes and brought to full fruition by F. Griffith, who finally defined twenty-seven such types fourteen years later.

He also worked with Andrewes on the classification by fermentative and other cultural properties of staphylococci, and in connexion with an outbreak of obscure illness in Hertfordshire from cases of which a *Neisseria* was recovered, he defined the fermentative characters by which members of this genus can be differentiated. Here the methods employed earlier in connexion with streptococci were applied with eminently satisfactory results: the capacity of meningococci, gonococci and other species of *Neisseria* to ferment glucose, maltose and other sugars are constant characters and now regularly employed in verifying their identity.

Studies of cerebrospinal fever

The bacteriology of cerebrospinal fever was one of the subjects which Gordon was commissioned to study by the Local Government Board, and his findings appear in *Reports* dated 1905-6 and 1907. The alarming outbreaks of this disease which occurred in overcrowded barracks in the first winter of the 1914-18 war led to his being commissioned as Lt-Col., R.A.M.C., and given full authority in everything pertaining to the investigation and control of this infection. In this work, which continued throughout the war, Gordon reached the zenith of his powers. He was the leader of an able group of workers, of whom T. G. M. Hine was his most constant associate throughout, whose classical studies form the main basis of present knowledge of cerebrospinal fever from every aspect except the therapeutic: chemotherapy, unheard of in those days and for long after, has now quite transformed this. They related the occurrence of this form of meningitis to the existence of a high meningococcus carrier-rate, and the latter to the circumstances of overcrowding, particularly in sleeping quarters. They defined four serological types of meningococcus and introduced treatment with monotypical antisera: this work was more directly Gordon's personal contribution to the study than any other, but he was also mainly concerned in the attempts to improve the efficacy of therapeutic serum and to devise a measure of this. Other aspects of the work were improvements in laboratory diagnostic methods, and particularly in media for cultivating the organism, and the organization and study of the mass disinfection of carriers by antiseptic steam inhalations. That such measures are now outmoded or superfluous in view of the facility with which meningococcal infection can be treated by other means does not detract from the merits of a monumental piece of work by the standards of forty years ago.

Studies of filtrable viruses

Apart from the time he spent at Millbank during the war, Gordon had worked continuously at St Bartholomew's Hospital throughout his career, and since 1908 had held the position of Assistant Pathologist, which involved both teaching duties and responsibility for hospital diagnostic work. In 1923 he resigned this appointment, retaining only the honorary title of Consulting Bacteriologist, in order to devote his whole time to research; he remained in the Department, but as a member of the external staff of the Medical Research Council.

The work to which he wished to devote his whole time was the study of virus diseases. This was not new to him: he had published two papers on poliomyelitis in 1912, two on mumps shortly after this, and one on the 'filter-passer of influenza' in 1922. The first item in his programme was a systematic re-study of the properties of a well-known virus, and he chose that of vaccinia. His monograph on the viruses of vaccinia and variola, published in 1925, is a classic. He showed how a characteristic animal lesion can replace the artificial cultivation used for bacteria as an indicator of the presence of living virus: since multiple local lesions can be produced with vaccinia in the rabbit he was able to do extensive quantitative work, determining the amount of virus in different vaccine lymphs, and the effect on it of heat, various disinfectants, gravity and filtration. He also studied different routes and methods of inoculation, the immunity resulting therefrom, the antibodies on which this depends, and the immunological relationship between the viruses of vaccinia, variola major and alastrim. The agglutination of virus particles from the crusts of a doubtful skin lesion by immune serum became on the basis of this work a method of verifying a diagnosis of smallpox.

Shortly after this he resumed the study of mumps, succeeding for the first time in transmitting this infection to monkeys, and contributed to the work on psittacosis which followed the discovery that this is a virus disease. This work filled an intervening period before his absorption in another problem which was almost wholly to occupy the rest of his working life.

Hodgkin's Disease

Mrs T. E. Rose, a relative of a member of the St Bartholomew's staff, whose daughter had died of Hodgkin's disease (lymphadenoma) had placed a fund at the disposal of the Medical School for research into the cause of this disease. A Research Fellow received the income for the greater part of the ten-year period to which this endowment was limited: towards its end there seemed no likelihood of a suitable further appointment, and a large sum remained unexpended. In these circumstances Gordon assumed direction of a team of workers, consisting mainly of colleagues but also including Dr Beatrice D. Pullinger, who played various parts in a concerted scheme for re-investigating the disease during the years 1928-29. Much of this work was ancillary to his own, which consisted of searching in affected tissue for different types of micro-organism, plentiful material being made available by the willing co-operation of the clinical staff. Among types of micro-organism systematically looked for were *Mycobacteria*, spirochaetes and fungi: at one stage the efforts of several workers were concentrated on the latter, 14 strains of which Gordon had recovered in culture from lymphadenomatous tissue. The eventual conclusion reached was that these must have been contaminants.

When the 'Rose Research' came to an end, Gordon continued this study alone, having by then reached the conclusion that the cause of lymphadenoma is a virus. Of his two main items of direct evidence for this, one was the production in rabbits by intracerebral inoculation with autolyzed tissue of a peculiar type of non-transmissible encephalitis. This phenomenon, generally known as the 'Gordon test', was later shown to be reproducible with other material containing eosinophiles, and the peculiar reaction in the rabbit's brain is now believed to be connected with a derivative of this cell and not with any hypothetical causative agent in tissues affected by lymphadenoma. His second finding, one on which all his subsequent work depended, was the demonstration in affected tissue of what were said to be virus particles, or elementary bodies. Having found these, he conceived the idea of treating cases of the disease with a vaccine consisting of these particles 'sensitized' by treatment with immune serum prepared in rabbits. It should be borne in mind in this connexion that in the first decade of this century and after, many people had believed in the feasibility of treating existing and even acute infections by means of vaccines, and Gordon himself had been a strong advocate of the use, particularly in severe haemolytic streptococcal infections, of a vaccine 'sensitized' by the method of Besredka, i.e. one which had absorbed the antibody from an immune serum.

Although clinical benefit was reported to follow this treatment for lymphadenoma, it has to be said that many of his colleagues and associates felt unable to follow him when his work had reached this point. It is almost superfluous to say that the demonstration of minute particles by prolonged staining with aniline dyes or by any other simple microscopic method is not, by modern standards, adequate evidence of the presence of a virus. It is perhaps significant that when Gordon embarked in 1923 on his first systematic study of a virus, he did so 'in the hope that immunological and, especially, serological methods of procedure that have done yeoman service in the past in the elucidation of diseases due to one or other of the well-known pathogenic micro-organisms might, possibly with some slight adaptation, prove capable of similar service in the present instance'. The policy of applying old methods to a new problem succeeded in this connexion, but when translated into a more difficult and obscure field it should perhaps have been supplemented by the use of more modern aids to the study of viruses. Gordon was a master of many methods employing serology or animal inoculation, but he never attempted cultivation of the chick embryo or possessed—or desired to possess—an ultracentrifuge, an electron microscope, or any of the finer tools which virus workers now employ.

In the last of his working years and during his retirement his mind was much occupied with viruses as the causes of other diseases than lymphadenoma. He described experiments suggesting that rheumatism may be so produced, and consistently supported the view, which he had held for many years, that viruses are concerned in the aetiology of cancer.

Honours: personal qualities

Gordon was an original member (1909) of the Army Pathological Advisory Committee, and after his service in the 1914-18 war remained Consulting Bacteriologist to the Army for many years. It may be presumed that his services to the Army were largely responsible for his appointment as C.M.G. in 1917 and C.B.E. in 1919. He was elected F.R.S. in 1924, and received the Hon. LL.D. of Edinburgh University in 1931.

He never held a University chair or readership, being content to serve at St Bartholomew's under the late Sir Frederick Andrewes, and at a time when he would have been given greater responsibilities in his department had he sought them, resigned from the staff to devote himself entirely to research. No man was ever more absorbed in his work: he took little interest in the average man's amusements, in money, or in honours, and never seemed to look forward to a holiday. This is not to say that he had no other interests: he was an archaeologist in the literal sense of loving everything that was old, particularly if it was also rare and of obscure origin or nature. He collected china, prints, and books, especially early medical classics, and had an extensive knowledge of history. When excavations were going on in the neighbourhood of St Bartholomew's he persuaded workmen to bring him fragments of pottery and glass, and was delighted with some iron pins which he believed to have been used to secure the shrouds on bodies in a plague pit. How varied was his knowledge may be gathered from his immediate identification of an object which had puzzled others as a tongue scraper.

The dominant feature of his temperament was enthusiasm. Over-optimistic as he may sometimes have been, he always seemed to be hot on the scent of some discovery, and much of the affection he inspired was due to the fact that he would wax as enthusiastic over some minor observation or project of a junior as over his own work. The encouragement as well as the endless help and advice which he gave to junior colleagues placed many of them including the writer in his lasting debt. His example and inspiration may well have induced some of them to follow a career in this branch of medicine. He was a beautiful technician, with a deft and almost loving way of handling his materials, and delighted in well-stained microscopic preparations and successful cultures. His creed was expressed in a short paper in the *St Bartholomew's Hospital Journal* in 1920 entitled 'The spirit of research': this well deserved re-publication in *The Lancet* in 1945, and can be read with profit by any research worker in any field. The following is its second paragraph:

'Research is its own reward. A genuine investigator should be equally immune to the praise or blame of his fellows. After all, he is the best judge. They can only see part of the picture. He knows the reverses, the weary months spent in working in the wrong direction, the failures, the mistakes, the scrap-heaps, the disappointments, and the snubs that Nature often administers to him with such vigour when he begins to feel unduly hopeful or elated. Nature, however, is never one-sided, and few pleasures are equal to that of a genuine discovery, however small and unimportant. Nor is this pleasure neutralised when such a discovery is found to have been made previously by somebody else. That gives confidence and actual encouragement to an investigator who is worth his salt. In course of time the investigator will come to realise that Nature is never wrong, that the fault—when fault there is—lies with himself, and thus he gradually comes to be immune to disappointment. Failures can interest him equally with successes; for, properly applied, a failure may sometimes be made to lead directly or indirectly to success.'

Gordon is credited with a high reputation as a raconteur, particularly of stories connected with odd characters among Oxford dons, even in his undergraduate days. In the years in which I knew him he drew on an apparently inexhaustible supply of anecdotes, told with immense gusto, often Rabelaisian but never offensive. He had an acutely developed sense of humour and particularly of the grotesque, and his company was sought for the sake of his conversation as well as his advice to an extent which must often have prolonged his working hours. He could afford this better than most, since he lived in a flat in Holborn which could be reached even on foot in ten minutes from his laboratory.

He married in 1916 Mildred Olive, daughter of Sir William Power, the first medical officer of the Local Government Board and the immediate author of many of the instructions for his earlier research work. She continued her work as an Inspector for the Local Government Board after their marriage. Her death preceded his by a few months: they had no children.

I wish gratefully to acknowledge the help I have received from Mr Ernest Gordon, Dr Gordon's only surviving brother, in writing of his earlier years: for further items of information relating to this period I am indebted to the Master of Marlborough, the Warden of Keble College, Mr J. B. Atkins, Lord Horder and Dr Bernard Klein. The Librarian of the Royal Society of Medicine, Mr P. Wade, very kindly provided the bibliography.

LAWRENCE P. GARROD

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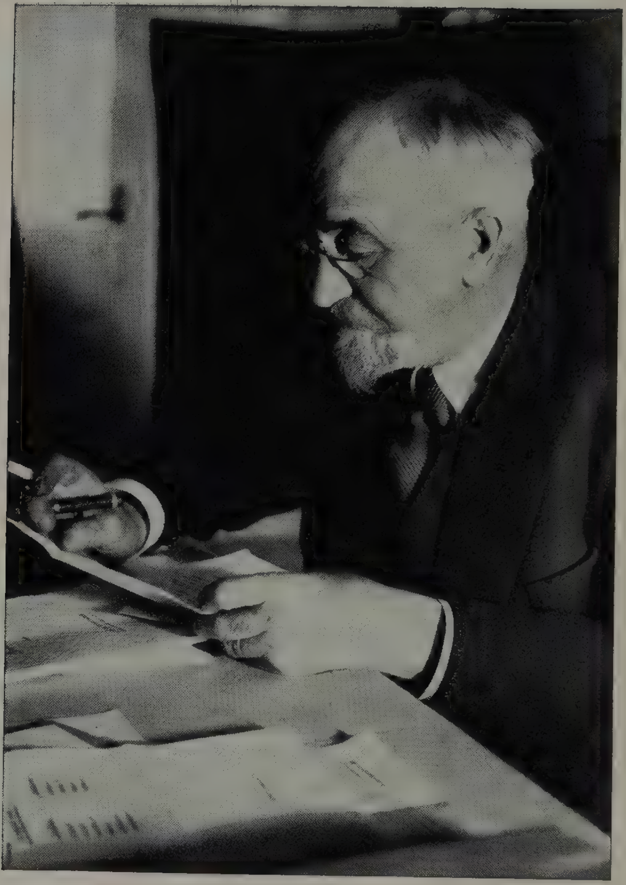


A. Supan

MAURICE LUGEON

1870-1953

Maurice Lugeon was born on 10 July 1870, at Poissy near Paris; but from 1876 onwards his home-town was Lausanne on the Swiss, that is the north, shore of the Lake of Geneva. Here he died on 23 October 1953, after several months of illness.

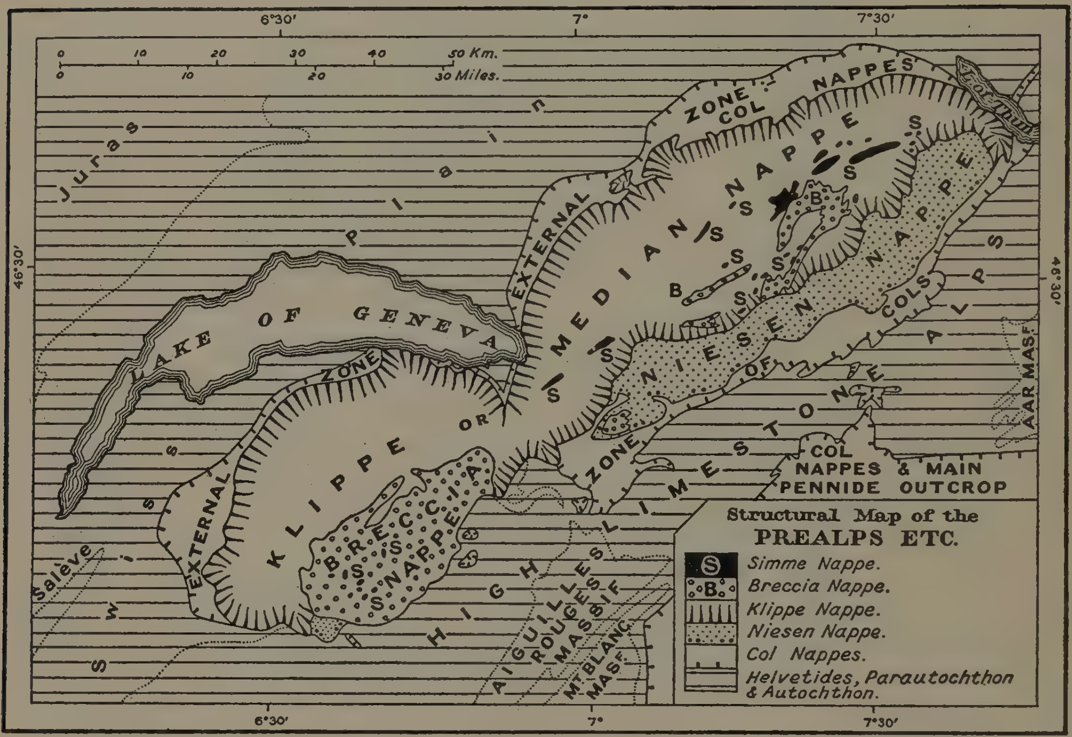
Lugeon has emphasized that two names, Chablais among places and Bertrand among persons, played a particularly important role in his life. Chablais is the district of France which faces Lausanne across the Lake of Geneva. Bertrand is the ever popular hero of French geology—Lugeon has styled him the 'Confucius of tectonics', that is of earth structure.

Chablais is mainly occupied by the south-western portion of what geologists call the Prealps, or more specifically the Préalpes romandes. Geographically these Prealps furnish an ill-defined and local border-zone to the Alps as a whole. Running north-eastwards, they leave French territory near the east end of Lake Geneva to continue through Switzerland to the Lake of Thun. Their total length is 120 km, and maximum breadth 40 km. To the north-west the Swiss Plain spreads out before them, mostly well below 1000 m, whereas their own summits often reach above 2000 m, while those of the High Limestone Alps immediately to the south-east rise clad in snow to over 3000 m (Dent du Midi, Diablerets, Wildhorn, Wildstrubel).

The Prealps are much more clearly defined in their geology than in their geography. Their Jurassic and Cretaceous formations are as distinct in facies, that is in character, from contemporaneous developments in adjacent districts as are the present-day inhabitants of Africa from those of Europe. In 1893 the solution of this extraordinary puzzle was suddenly recognized; and, as we shall see, Lugeon played a part in alerting Schardt, the actual author of the discovery. The Prealps are a complex exotic mass which has travelled into position from distant southern sources. Where they now stand they hide local formations under foot. They are a complex mass in that they consist, not of a single mighty displaced sheet of rock, but rather of a number of such sheets, partially superimposed, each with its own particular facies. The principal sheets, or nappes, have been named from above downwards as follows (though the confusion is sufficient to render it doubtful whether the Simme Nappe should not be better classed as intermediate in position between the Breccia and Klippe Nappes):

Simme Nappe,
Breccia Nappe,
Klippe Nappe,
Niesen Nappe,
Col Nappes.

In our map the Prealp nappes are variously ornamented, while the lower less-travelled or untravelled structural elements are distinguished by horizontal ruling.



With this introduction let us return to Lugeon. In 1885, when only fifteen years old, he began to accompany an assistant surveyor engaged in preparing the official geological, 1:80000 map of the French portion of the Prealps. He soon learnt the jargon of Flysch, Red Beds, Lias, etc.; and in 1887 he published his first paper, dealing with a Mid-Tertiary flora he had found in an excavation at Lausanne. By 1891 he was quite an author with six papers to his credit. By this time too he had advanced to the position of rucksac-carrier to Renevier, Professor at Lausanne, who, though a Swiss, was chief surveyor in the Chablais district.

As often in complicated country, differences of opinion arose in regard to the age of particular formations. The Breccia Nappe, more often called the Chablais Breccia Nappe, has outcrops both in the French and Swiss portions of the Prealps. One of its component formations consists of dolomite and gypsum, very like what is found in the Trias of neighbouring districts. Renevier, following Alphonse Favre, correctly placed this dolomite-gypsum formation in the Trias. On the other hand his colleague Auguste Jaccard, Professor at Neuchatel, preferred to call it Mid-Jurassic; while Hans Schardt and Ernest Favre, working in Switzerland, for a time thought it was probably Early Tertiary.

The difference was characteristic. Renevier paid little attention to structural relations, and in absence of fossils was prepared to recognize a formation by its lithological character, however difficultly placed it might appear to be. Schardt on the other hand had a clear perception of structure. He saw that the Chablais dolomite overlay Tertiaries on a wide scale, and he therefore sought to interpret it as itself a Tertiary formation, disguised in curious Prealpine fashion. In 1891 Schardt abandoned this view, and even admitted that the associated Chablais Breccia, a great sedimentary conglomeratic breccia-formation, might be Jurassic. All the same he seems to have continued rather muddled, for his somewhat tentative change of stratigraphical outlook did not lead to corresponding tectonic enlightenment —nor indeed did Jaccard's confident assignment next year of the Breccia to the Jurassic.

Let us here return to Lugeon. Michel-Lévy, Director of the French Geological Survey, had become alarmed at the differences of opinion noted above, and in 1891 came for a field-conference with Renevier and Jaccard, accompanied as it happened by young Lugeon. The latter seems to have taken an active part in the 'vast discussions' that followed, for, to the chagrin of Renevier, Michel-Lévy carried him off alone to try his mettle in the neighbourhood of St Gervais; and also, without a word to Jaccard, asked him to prepare a report on the massif of the Chablais Breccia.

Next year Michel-Lévy called in Marcel Bertrand to share in the mapping of Chablais, allotting him the Môle, a mountain on the western edge of the Prealps. Bertrand asked Renevier to introduce him to the local stratigraphy, and so the two set off on 22 June 1892, with Lugeon in attendance. Bertrand, though 45 years old, was still full of gaiety and high spirits, and proved very congenial company for Lugeon in contrast with the water-drinking, non-smoking, solemn Renevier. Before reaching Taninges, Renevier had turned back; and Lugeon took his new friend to see an exposure which the previous year he had thought showed Coal Measures on Carboniferous Limestone. When they arrived they stood utterly astonished, stupefied. The limestone had all the appearance of Upper Cretaceous (since confirmed).

'Try,' says Lugeon, 'to carry yourselves back to the antenappist epoch. If one found tomorrow granite on top of the Eocene of the Paris Basin, one's stupefaction would certainly be less than that into which Bertrand and I were on this occasion plunged.' Lugeon's exclamation of amazement is very revealing. Prealpine geologists had got fairly well accustomed to seeing Trias on Tertiary, but Coal Measures on Upper Cretaceous was a new combination, and it reawoke the instinct of wonder.

We must pass on. Lugeon's work on the Chablais Breccia, which had been started in 1891, roused Michel-Lévy to such enthusiasm that he brought his family to the district in 1892 and 1893 to follow the results in person. In the latter year Lugeon published five papers on Chablais geology, two of them with Renevier. He also, again associated with Renevier, helped to lead an excursion of the Swiss Geological Society, which was attended by, among others, Michel-Lévy, Bertrand, de Margerie, Kilian, Haug and Duparc. Battles royal ensued. Lugeon established to everyone's satisfaction that the Chablais Breccia is indeed Jurassic. He also demonstrated that in the important area overprinted with the name Breccia Nappe on our map this Chablais Breccia with underlying Trias rests peripherally almost everywhere on Tertiary strata. For Lugeon, in his youth and inexperience, the structure seemed to be a mushroom-fold, in which older rocks were supposed to have risen centrally through younger, thereafter to spread out in all directions. Here Lugeon made a structural mistake. Bertrand, who in good humour had been chaffing the leaders and others as they went along, now spoke in serious vein: 'Perhaps one day it will be said that the Breccia massif is a *lambeau de recouvrement* a little larger than the others.' By this he meant that the outcrop under discussion very probably 'floated' completely isolated on a foundation of younger formations, without the imaginary mushroom root that Lugeon postulated.

Lugeon in reminiscences of Marcel Bertrand published in 1951 reports as follows: 'The magnitude of this hypothesis surpassed our comprehension, so that no one at the time paid it any attention; but, when I repeated it some days later at Lausanne before the Swiss Geological Society, including Albert Heim the defender of the Glarus Double Fold, there was one there, Hans Schardt, who adopted it, and who a few months later put forward the idea of the travel of the Prealps without a word of Bertrand's hypothesis, without mention of what I had said in his presence. Several times [in after years] Marcel Bertrand, a little bitterly, told me how much he regretted not having insisted on immediate publication, along with myself, of this his vision. But, he added, he had not wished to influence me in regard to the idea which I had developed, perhaps with reason, regarding peripheral over-turning in the famous mushroom-fold and in a neighbouring double fold in the Val d'Illier, similar to that of the Glarus mountains. To call in question the Glarus Double Fold was heresy sufficient to bring one to a funeral pile in the Place de Grève! Naturally Bertrand did not wish to precipitate me into controversy.'

Thus Lugeon to the end felt that Schardt had wronged Bertrand in his 1893 reinterpretation of the Prealps. Schardt was a difficult man, who quarrelled with many of his contemporaries; but there is good reason to believe him entirely innocent in this particular connexion. It is pleasant, for instance, to read in his 1893 announcement: 'The savant who above all brought home to us this phenomenon [large-scale overthrusting], of which he already spoke some ten years ago, is Marcel Bertrand; and one can well say that in its demonstration M. Bertrand has acquired a merit equal to that of Charpentier when the latter established the theory of the glacial period.' This statement errs rather on the side of exaggeration, if we think of earlier and contemporary research in Switzerland, Quebec, Belgium, Scotland and Scandinavia; but at any rate it is a generous tribute. True, there is no allusion to Bertrand's conversational criticism of Lugeon's mushroom; but anyone who has followed closely Schardt's self-education must realize that by 1893 he (Schardt) scarcely needed prompting in this matter—beyond being convinced by Lugeon's detailed field-work of the Jurassic age of the Chablais Breccia. Schardt right up to the spring of 1893 had developed structural interpretations of the Klippe Nappe, which came very near in style to Lugeon's mushroom; but they were not so extreme, and the mushroom itself proved more than he could swallow. He knew the Swiss outcrops of the Breccia Nappe, 30 to 60 km north-east along strike from the main Chablais outcrop. In them Trias and the Chablais Breccia occur in patches, large and small, obviously 'floating' on Tertiary. No, said Schardt, if the main Chablais mushroom is not in itself incredible, at any rate a whole crop of mushrooms is absurd. Schardt had struggled with Prealpine problems for some fourteen years. Bertrand had come to the district only the year before. Once Schardt knew for certain that the amazing Chablais Breccia was Jurassic, he realized, what apparently escaped Bertrand, that its utterly extraordinary facies, combined with its structural position, bespoke travel from a distant source, and also that an extension of the same principle solves the riddle of the Prealps as a whole.

Bertrand's paper of 'some ten years ago' (1884), referred to by Schardt, was, in the main, a brilliant and well-merited criticism of the prevailing Escher-Heim conception of the Glarus Double Fold. In the text Bertrand devoted two sentences to the Prealps, and in the accompanying map marked all their Mesozoic outcrops as *lambeaux de recouvrement*. Quite apart from what we have already discussed, various writers have claimed that Schardt's theory plagiarizes this early presentation. When, however, we look into the facts we find ourselves immersed in a comedy of errors:

(1) Bertrand deserves little or no credit for this particular aspect of his 1884 paper. He had not visited the ground and he merely suggested that the Mesozoic outcrops of the Prealps overlie the local Tertiary because they make most of the high ground. Actually, however, the Mesozoics as often as not pass underneath associated Tertiary.

(2) Schardt, in the longer of his two 1893 papers does in fact refer to Bertrand's 1884 idea of probable *nappes recouvrement* in the Prealps; but he makes the strange mistake of imagining that Bertrand pictured southward nappe-travel. Most emphatically Bertrand, like Schardt, thought the travel had been northwards. Schardt was misled by Bertrand's introduction of a Belgian mining term, *cran de retour*, which he (Schardt) misunderstood. This does show careless reading, but not malice.

(3) Bertrand in a letter to Schardt in 1897 wrote: 'As for the Prealps, I never dreamt [in 1884] to see in them the product of an overthrust, and I

think I ought to add that before your own subsequent work, and that of Lugeon, such an idea would have been veritable folly.' All one can say is that by 1897 Bertrand had forgotten part of what he had published in 1884.

The records of the 1894 session of the International Geological Congress at Zurich reveal no reaction to Schardt's theory of the Prealps, beyond sections and notes supplied by Schardt himself. Bertrand was there; but he was mainly concerned with comparison of the rhythmic development of the great mountain chains of the world, to which Lugeon listened in ecstasy. It is symptomatic of the meeting that Heim in an account of the Glarus Double Fold did not think Bertrand's 1884 paper worthy of a place in his selected bibliography.

Lugeon meanwhile had spent a couple of terms at Vienna studying palaeontology under Karl Zittel; but Michel-Lévy at Zurich decided to bring him to Paris to facilitate supervision of his Chablais memoir. So we find the young recruit duly installed in a little room of the Geological Survey, with the Director calling on him twice a week to check up on progress. Bertrand also looked in, but his visits were more or less surreptitious. Michel-Lévy was conservative and wanted to hear nothing of *nappes de recouvrement.* Bertrand contrariwise egged Lugeon on. Michel-Lévy would demand rubber to limit underground suggestions. Bertrand would complete a broken line regardless of prudence. Once the two great men arrived together, and before long Lugeon, helpless, saw his Chablais drawing disappear under rival versions of the tetrahedral theory of the earth.

Besides benefiting from close conversation with Bertrand, Lugeon attended his lectures at the School of Mines, and enjoyed his frequent exchanges with Haug at the Geological Society. Haug, with Alsatian violence and admirable dialectic, was hard to vanquish. He thought little of Bertrand, and constantly advised Lugeon not to waste time on his lectures.

Meanwhile geology progressed. Schardt's 1893 announcement after a little delay stirred up passionate controversy, which, though short-lived, exceeded in printed bitterness anything that Scottish Highland interpretation has provided during a century. Lugeon was one of the first to desist from attack. Writing in 1895 we find him closing his great memoir on the 'Région de la Brèche du Chablais' with the words: 'It is with doubt, mingled, however, with conviction, that I finish this work.' Next year he declared himself completely satisfied.

This takes us to 1896 when Lugeon was appointed Privat-docent at Lausanne University. The same year he spent a stimulating week with Bertrand and Henri Golliez while these two successfully investigated the region between Glarus and the Prealps to clarify structural relationships. Then in 1898 Lugeon was promoted Professeur extraordinaire, a post which he held until 1906 when he succeeded Renevier as Professeur ordinaire.

Lugeon's conversion and Bertrand and Golliez' collateral support produced a great impression on those who watched the Alpine struggle. This was enhanced in 1900 by Lugeon's discovery of a root in the Rhone valley for the lowest of the Prealpine nappes. The locality is included in our map just above the index in the area overprinted 'Col Nappes and Main Pennide outcrop'. For many it seemed that Lugeon had succeeded in bringing the nappe interpretation of the Prealps down from the clouds to solid earth.

The thirty years following 1893, tempestuous in early stages, saw the most exciting rush of fruitful research that has ever stood to the credit of geology. The field was the structure of the Alps; the participants, French, Swiss, German, Austrian, Italian. In distinguished company Schardt and Lugeon, both now superb tectonists, led the van. Contributions came from every side. Among them three, perhaps, may be singled out for special mention: G. Steinmann's realization of the Simme Nappe in the Prealps, 1906; E. Argand's conquest of the Pennines, south of the Prealps, 1911, 1916; and Termier's unveiling of the Window of the Hohe Tauern in the Austrian Alps, 1903. Steinmann was one who came to curse and stayed to bless. Argand and Termier were definitely inspired by Lugeon.

Argand's relations with Lugeon were particularly intimate. Abandoning a training for medicine he came to Lugeon in 1905 as a most exceptional student. For the next seven years the two worked in close collaboration, until in 1911 Argand was persuaded to take the chair at Neuchâtel in succession to Schardt who followed Heim at Zurich. Two joint notes by Lugeon and Argand in 1905 introduced a new attack upon the Pennines; but later Lugeon generously refused to be drawn into co-authorship on this subject, for he wanted all to realize the extent of Argand's originality.

Lugeon's greatest personal triumph came with his delivery, before the Geological Society of France, of a paper entitled: 'Les grandes nappes de recouvrement des Alpes du Chablais et de la Suisse.' The occasion was a *réunion extraordinaire*, 3-11 September 1901, at Lausanne and in Chablais. Most of the reunion was devoted to excursions in the Prealps, but the above-mentioned paper, delivered on the last day, covered all that was known of nappe structure, not only in the Prealps, but also in the High Limestone Alps and even the Pennines. Lugeon repeated this account next year on 17 February at a session in Paris. As might be expected he included a reinterpretation of the Glarus Double Fold in Bertrand's style. Heim who received a proof, sent an open letter in reply, dated 31 May 1902. In it he frankly confessed that at last he was strongly inclined towards the Bertrand interpretation, and that he hoped to renew his studies in the district and share in the joy of further discovery. Next year at the 1903 meeting of the International Geological Congress in Vienna, Lugeon continued with an address on 'Les grandes nappes de recouvrement des Alpes suisses'. On this occasion Heim was present, and electrified his audience with a full enthusiastic acceptance of the new views. Heim's conversion was comparable in its effect upon the outside geological world with Geikie's 1884 change of front in relation to the North-West Highlands of Scotland.

Lugeon worked in all parts of the Alps, but he devoted particular attention to the High Limestone Alps of the Valais, which he mapped in great detail and described in parts 1-3 of a special memoir of the Swiss Geological Survey, 1914-18, entitled: 'Les Hautes Alpes calcaires entre la Lizerne et la Kander'. We have already mentioned his discovery of a Prealp root in this region. He also demonstrated that, though the High Limestone nappes on the whole underlie the Prealp nappes, they also on occasion rise and intrude themselves, as it were, into their tectonic cover. Lugeon's introduction of a wealth of apposite detail reveals true genius, alike of observation and exposition. His story keeps us constantly aware of the fascination of the subject and of the grandeur of its setting. It also conveys some small suggestion of the hardship and danger faced by a solitary investigator in such environment.

By this time the climate of geological opinion had undergone an overwhelming change. Lugeon naïvely remarks that it had begun to rain nappes, and that anyone visiting a mountain expected to find a new nappe that he could claim as his own.

Outside the Alps Lugeon's most notable tectonic papers concerned the Carpathians, 1903, and, with Argand, Sicily, 1906. In addition his work on applied geology brought him international renown as a consultant on dam-sites; so much so that in certain circles he is known as the 'père des barrages'. In 1909, a group of French engineers had persuaded him to study a project on the Rhone at Génissiat below Bellegarde, which occupied much of his time for the next twenty years. He also participated in the Donzère-Mondragon scheme, much further down the Rhone, and in many others, some of them in Africa and the Americas. His book, *Barrages et géologie*, 1933, Lausanne and Paris, is a classic based on his rich personal experience.

Anyone who has helped to trace major structures in a folded mountain chain finds himself faced primarily with the question of *what* has happened. He is, however, conscious all the time of supplementary questions of *how* and *why*, *when* and *where*. To these supplementary questions Lugeon in his later years devoted increasing attention, following an example set by Schardt and Argand. It is a field of thought where extrapolation from fact to fancy is of necessity embarrassingly free, but Lugeon's qualifications as a tectonist and as an associate of engineers render his opinions worthy of close consideration. In publications of 1940 and 1941, in part with D. Schneegans and E. Gagnebin, he attempts to separate effects determined by the pull of gravity from others more immediately due to external push—the latter ascribed to Taylor and Wegener's continental drift. Lugeon, for instance, excluded the Jura mountains from the push category. His argument here is reminiscent of A. J. Bull's 1928 Presidential Address to the Geologists' Association; but it has features all its own. Other related suggestions appear in a note on 'diverticulation', 1943, a digression on the Morcles Nappe, 1947, and a contribution to Charles Jacob's jubilee, 1949. In the last-mentioned, Lugeon accepts unreservedly an interpretation of 'collapse structure' given by J. V. Harrison, N. Leslie and N. L. Falcon in regard to a well-known exposure in Iran.

Scottish geologists remember a very happy visit by Lugeon to the North-West Highlands in 1912. He was one of a party including among others Barrois, Haug, Heim, Jérémine, Leith, Pruvost, Reusch and Tietze. The leaders were Peach and Horne, following a British Association meeting at Dundee, where Peach had presided over the Geological Section. In commemoration Lugeon composed the words and music of 'La Chanson du Moine Thrust', and had it printed for private circulation. The song tells how the Moine Thrust, if it had but wished, might have covered the whole earth and even annexed the moon in the name of Scotland; but to the sorrow of the good Dr Horne it had found 'Peach avec sa panse pleine un peu lourd à porter.' When in 1938 Lugeon received the Wollaston Medal at the hands of the Geological Society of London, he recalled this incursion into the realm of poetry and music adding: 'Dans cette direction de l'esprit, je n'ai jamais renouvelé un pareil incident dans mon existence.' On his Wollaston visit Lugeon found time to examine the outposts of the Alps deployed on the Dorset coast.

Lugeon was appointed Honorary Professor at Lausanne on his retirement in 1940. His other honours at home and abroad are far too numerous to recite on this occasion, beyond the following brief selection: 1920, Correspondant de l'Institut de France; 1936, For. Mem. R.S. Edinburgh; 1937, Grand Officier Lég. d'hon., France; 1938, Wollaston Medal, Geol. Soc., London; 1944, For. Mem. R.S. London. His appointment as Grand Officier Légion d'honneur was made on the occasion of a University celebration of the 50th anniversary of his first scientific publication.

Lugeon is survived by his widow Mme Ida Lugeon-Welti and a son Jean, Director of the Swiss Meteorological Office, Zurich.

Notes et Publications de Maurice Lugeon, Lausanne, is in the main a list of Lugeon's works.* It has been brought up to date on a number of occasions, mostly celebrations of important anniversaries. The sixth edition, 1950, marked Lugeon's 80th birthday and includes 292 entries. Two later valuable communications will be found in 'Réunion géologique en Provence pour commémorer le cinquantenaire des oeuvres de Marcel Bertrand, 28 Septembre-6 octobre 1950', *Ann. Fac. Sci., Marseille* (2), **21**, 1951. They furnish, not only welcome reminiscences of Bertrand, but also a defence of two of his tectonic interpretations in Provence, which have been exposed to ill-founded criticism.

I should like to thank Professor M. H. Badoux of the University of Lausanne for the loan of the half-tone block of the portrait of Maurice Lugeon; also the Clarendon Press for permission to reproduce the map from my book *Tectonic Essays: Mainly Alpine*, 1935.

E. B. BAILEY

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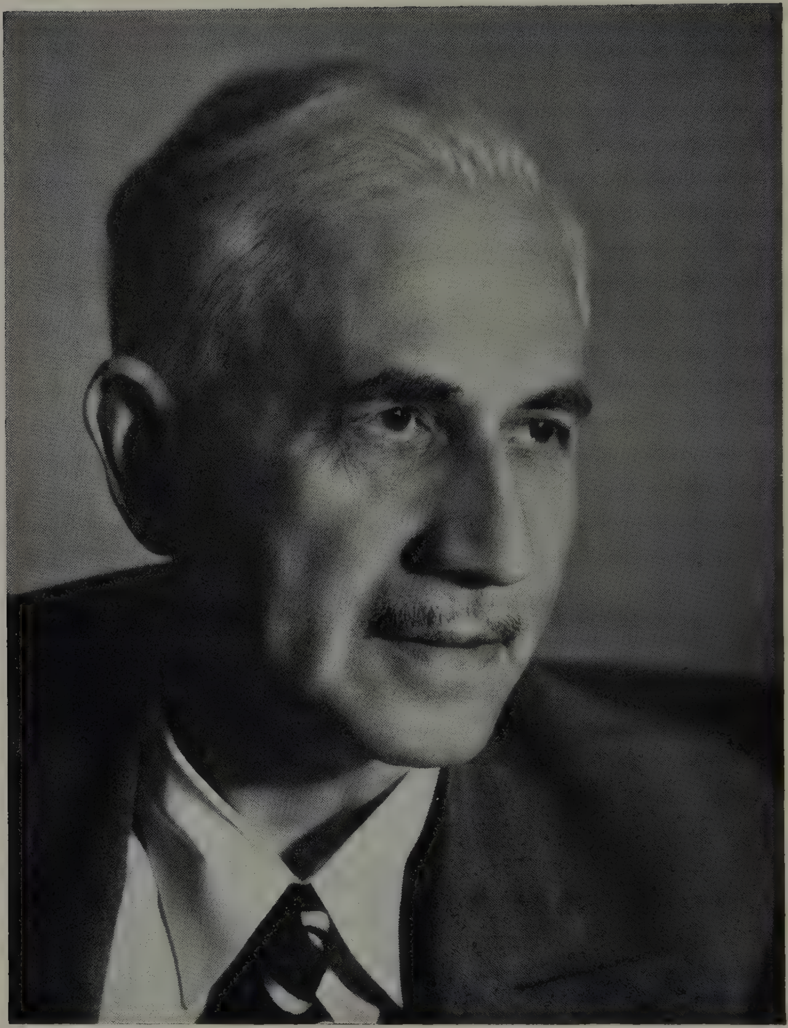
* A complete list with 299 entries is promised in the *Bulletin* of the Geological Society of France.



Otto Meyerhof

OTTO MEYERHOF

1884-1951

Otto Meyerhof was born on 12 April 1884 in Berlin and died in Philadelphia on 6 October 1951 at the age of 67; he was the son of Felix Meyerhof, who was born in 1849 at Hildesheim, and Bettina Meyerhof, *née* May, born in 1862 in Hamburg; both his father and grandfather had been in business. An elder sister and two younger brothers died long before him.

In 1923 he shared the Nobel prize for Physiology (for 1922) with A. V. Hill. He received an Hon. D.C.L. in 1926 from the University of Edinburgh, was a Foreign Member (1937) of the Royal Society of London, an Hon. Member of the Harvey Society and of Sigma XI. In 1944 he was elected a member of the National Academy of Sciences, U.S.A.

Otto Meyerhof went through his school life up to the age of 14 without delay, but there is no record that he was then brilliant. When he was 16 he developed some kidney trouble, which caused a long period of rest in bed. This period of seclusion seems to have been responsible for a great mental and artistic development. Reading constantly he matured perceptibly, and in the autumn of 1900 was sent to Egypt on the doctor's advice for recuperation. In this trip he was accompanied by a young physician, a member of his father's family (Dr Max Meyerhof) who was about 10 years his senior. This trip which lasted four months appears to have had a great influence on his further development. When home he finished his last years of school and final examination in Berlin with far less strain than before his journey.

He was evidently much influenced by the close friendship and mutual understanding that developed between his mother and himself during the months of his illness and which lasted until her early death in 1915; during these years he started to write poetry. After leaving school he began studying medicine, in Freiburg, Berlin, and later in Strassburg and Heidelberg, and took his medical degree in 1909. During his first term as a student in Berlin his time was evidently much taken up with problems of adult education and he initiated friends' organizations for evening classes for labourers in Berlin which were continued for several years. At Heidelberg he first met Hedwig Schallenberg who became his wife in 1914 and who survived him; she was killed in a motor accident in 1954; they had three children; a son and daughter now in the U.S.A. and a son now in Canada. Mrs Meyerhof's sweetness of temperament and devotion to her family, together with her keen interest in art, provided the stable background to Meyerhof's life.

In Heidelberg he occupied himself with psychiatry for his doctorate, obtained in 1909; and he was first importantly concerned with psychology and philosophy. In this time he published a book *Beiträge zur psychologischen Theorie der Geistertörungen.* With his psychological and philosophical studies this notice will not be concerned. His attention seems to have been directed first by Otto Warburg (then in Heidelberg, later in Berlin-Dahlem) towards physiology and to problems of cell physiology, and this association proved important for his career. After he had worked for a short time with Bredig in Heidelberg upon physico-chemical studies at the Heidelberg Klinik and at the Zoological Station at Naples, he went to the physiological laboratory at Kiel in 1912, at first under Professor Hans Bethe, from 1915 under Professor Rudolf Höber: in 1918 he became assistant professor there.

In 1924 the Kaiser-Wilhelm-Gesellschaft asked Meyerhof to join the group in Berlin-Dahlem; in this way he became directly associated with such figures as C. Neuberg, F. Haber, M. Polanyi and H. Freundlich. In 1929 he was invited to take charge of the Physiology Department of the new Institute for Medical Research in Heidelberg, where he had R. Kuhn, K. W. Häuser and R. Krehl as colleagues. A most fruitful period followed; but gradually after 1933 conditions became too difficult; he was not even given permission to attend the International Physiological Congress in Zürich in 1938, and in this year he left Germany with his wife and family. Until 1940 he worked as Directeur de Recherches at the Institut de Biologie physico-chimique in Paris, where he received support from the Josiah Macy Jr. Foundation.

The details of his subsequent history were given by Dr A. N. Richards in the Memorial meeting at Philadelphia. In June 1940 he and his family drove by taxi to Toulouse where he found friends in the Medical Faculty. This could not be for long and escape became essential. From now on, the history of the family's flight from France reached the high-light of tragedy shown by large numbers of refugees, and could be narrated similarly for many Dutchmen and others. Eventually, by the efficient and kind help of the Unitarian Service Committee, they escaped over the Spanish frontier; but it seemed that their fate was for long in the balance, and that had it not been for the tactful and persistent arguments of Mrs Meyerhof they would have been sent back to France. Eventually, they reached the U.S.A. via Lisbon in October 1940, where a post in the University of Pennsylvania had been created jointly by the University and by the Rockefeller Foundation; so Meyerhof spent the last years of his life in the Department of Physiological Chemistry, directed by Wright Wilson. He found many old pupils in the U.S.A. who rallied to help him. During the summer months, he mostly worked at the Marine Laboratories, Woods Hole, where he had his first heart attack in 1944; this he surmounted through the devoted care of his wife. His second fatal attack came suddenly in the midst of active plans for the future.

As the writer of this obituary only had the privilege of meeting Otto Meyerhof for short times on four occasions, Dr H. Blaschko, one of his co-workers now in Oxford, has kindly provided the accompanying note.

'In 1923 Meyerhof received the Nobel Prize (it was the prize of 1922) and soon he was installed at Dahlem. He had been offered the Chair of Biochemistry at one of the great American Universities (Johns Hopkins?), and in order to secure his stay in Germany, the heads of the various departments at the Kaiser-Wilhelm-Institute for Biology at Dahlem (Correns, Goldschmidt, Warburg, Hartmann and Mangold) approached the Kaiser-Wilhelm Society; they said they were willing to give up one room each if the Society agreed to create a new department for Meyerhof. The Society raised the necessary funds, and within a few weeks Meyerhof was at work in Dahlem. His department suffered from the disadvantage that the rooms were scattered over three different floors of the building, but on a smaller scale there were already all the elements which were present in his much greater and beautiful institute at Heidelberg. Lohmann was there from the start, and so were the two assistants, Schulz and Schröder. The latter two are still at work in the institute in Heidelberg, together with Mohle who joined them upon arrival in Heidelberg. This continuity of the staff was a great asset for the establishment of a laboratory tradition: visiting scientists were quickly assimilated and they lost little time in the preliminary stages of their stay.

'Their main introduction into the spirit of the laboratory however, came from Meyerhof himself and the example set by him as an experimenter. It is as an experimenter that we remember him first and foremost. He was forced, by the fertility of his own work and that of his colleagues as well as by his own theoretical interests, to spend a good deal of his time at his desk, but one got the impression that he was always impatient to get back to the bench. His pupils remember how the morning's work was first discussed with Schulz; then he used to return to his office only to hurry back at intervals to the bench: he was always there in time for the start of the experiment proper. His own experiment always remained for him the most important part of the day's work; this is what we remember more than anything else. At first sight this might appear surprising, as by temperament and by training Meyerhof was interested in theory and interpretation. It is this central importance of the experiment that Meyerhof's pupils took away as the most important influence of the Meyerhof tradition. But there were other factors which explain why his laboratory became such an important training field. In spite of his pre-occupation with his own work he daily saw each of his younger colleagues to discuss the day's work. And then there were occasions when in his office the general plan of the work was reviewed. Often these talks took up more of his time than he had originally intended; he was often anxious to get back to his own experiment. But when his attention had been caught time did not matter. These are the occasions that stand out in one's memory. Here we were able to contribute our own ideas, but also to profit from his constructive suggestions and his critical ability. Already at Dahlem, Meyerhof had started informal colloquia for the small group of his colleagues; in Heidelberg there was also the colloquium of the Institute as a whole, to which visitors were invited. The writer remembers to have met some of Professor Embden's younger colleagues at these occasions; Frankfurt was close enough for them to accept an invitation to these colloquia.

'Meyerhof's critical attitude did not prevent him from readily discarding outworn interpretations, however important, and accepting new ones supported by adequate evidence. The writer recalls vividly an experience when he visited Heidelberg in December 1929. Meyerhof was just beginning work in the still unfinished new Institute. The floor boards had not yet been laid down and the benches had to be reached on insecure planks, but since gas and water had been installed, experiments were in full progress. Meyerhof was impatient to start work. Later, in his also unfinished office he told of Lundsgaard's letter describing his still unpublished experiments, according to which lactic acid production was not essential to muscular contraction. He was expecting Lundsgaard in the laboratory in the coming month, but although reserving final judgment until Lundsgaard's arrival, he was quite prepared to believe that Lundsgaard was right. The present writer, who earlier on had witnessed Meyerhof's critical attitude to Embden's experiments on the time course of lactic acid formation, still remembers his surprise at the readiness to consider these new and—to Meyerhof—still unverified observations.

'The Heidelberg Institute was ideally suited for experimental work. Even today, after 25 years, it impresses the visitor as a beautifully planned building. Meyerhof required much specialized equipment, but as he was never content with existing methods there was never any finality in the experimental set-up. The Institute was well-adapted for these needs.

'Although not endowed with great manual skill, Meyerhof has been responsible for many experiments which require delicate manipulation. Almost invariably he contributed the basic idea for a new apparatus which then found its final shape in the hands of his skilful helpers, Schulz and Möhle.

'Meyerhof made full use of the laboratory and its resources, but he was aware of the fact that equipment was not everything. Foreign visitors who had admired the Institute were sometimes at the end of their tour sent away with the suggestion to visit, before leaving Heidelberg, also the Zoological Laboratory at the University; there they would see that first-rate research could also be carried out under most primitive conditions. The Chair of Zoology at the University was then held by Herbst, for whom Meyerhof had great regard.

'Meyerhof was always well-informed about progress in other fields of knowledge. It remains difficult to understand how in spite of a delicate constitution he was able to achieve so much. This was only possible because he combined great curiosity with tenacity which made him often in his work, disregard physical discomfort. This too became a great educating experience for those around him: the wish to improve in tomorrow's experiment the result obtained in today's work; that was the main driving force and the main content of his teaching.'

The work of Otto Meyerhof and of those directed by him in all cases reached such a high and critical standard of scientific concept and execution that it would be somewhat invidious to pick out in a bibliography for special mention work from the large series of papers which have been published by himself and his pupils. Hence the bibliography attached includes papers emanating from his school as well as those to which his own name was added: this gives the most complete picture of the total scientific effort. Of other bibliographies the most extensive was that collected by Dr Nachmansohn in the Obituary written by Alexander v. Muralt, *Ergebn. Physiol.* **47**, 1952; other valuable information about his life is to be found in the article by D. Nachmansohn, S. Ochoa and F. A. Lipmann (*Science*, **115**, 363-369, 1952) and by D. M. Needham (*Nature*, **168**, 895, 1951).

It is clear in any case that in a short statement it is not possible to enter into any elaborate analysis of individual papers. If the writer was forced to make a selection of the two most important achievements of Meyerhof he would select first the definite proof that in isolated but otherwise intact frog's muscle, lactic acid formed was actually reconverted to carbohydrate under the influence of oxygen. This biochemical study decided at once between the two theories, (*a*) that lactic acid was completely burned and, (*b*) that lactic acid was restored to the muscle as carbohydrate. It confirmed A. V. Hill's deduction (1914) from his observation that the heat liberated in the processes of contraction and recovery was not sufficient for the complete combustion of the lactic acid formed. About four-fifths of the lactic acid was restored to carbohydrate in the muscle at the expense of the burning of the other one-fifth part. It was this achievement which led the Nobel Committee to recommend for the Nobel Prize of 1922, A. V. Hill and Otto Meyerhof.

The next most important achievement was the preparation of a KCl extract from muscle capable of carrying out all the steps of glycolysis with added glycogen or hexosedi-phosphate; in the presence of another factor, obtained from yeast and known as hexokinase, glucose was also glycolyzed in the system. It was essentially this type of biochemical preparation which has been used both in Meyerhof's studies and those of others since this time; upon this experimental finding the modern proof of the Embden-Meyerhof theory of glycolysis may be said to rest. Other important discoveries arose from the isolation of adenosine triosephosphate by K. Lohmann in Meyerhof's laboratory. Very important too was the discovery, made independently and about the same time by Needham and Pillai, and by Meyerhof, of the coupled oxidation and phosphorylation of the muscle extracts.

Turning to a more detailed analysis; broadly speaking Meyerhof's work started with respiration changes in the sea urchin's eggs, blood cells, various bacteria; also the effect of narcotics and methylene blue on oxidation processes. This led him to the discovery of a respiratory component, needed both for glycolysis in animal tissues and for alcoholic fermentation, and brought him close to the sulphydryl field, to which Hopkins made his brilliant contribution in 1921 with the isolation of glutathione. In 1911 a publication appeared upon the heat production during oxidation changes in eggs, and one with O. Warburg upon the respiration of killed cells and cell fragments (1912). For several years he was interested in various aspects of the respiration of yeast and other cells. Three papers appeared upon the nitrifying bacteria. These fundamental observations were soundly grounded in energetics and evidently prepared his mind for entry into his main life's work towards the end of World War I. It was characteristic of him that while pursuing his problems as deeply as they would go in biochemical and physico-chemical analysis, he never allowed himself to forget that the ultimate aim was to integrate them with the phenomena of life.

To understand the original contribution, some knowledge of the background is necessary. W. M. Fletcher's observations at the turn of the century upon CO_2 evolution in surviving muscle had killed the somewhat mystical inogen theory and opened the way for more positive studies. The work of Fletcher and Hopkins (1906-7) then opened a new phase of interest in muscle tissue with accurate observation upon the relation between the physiological state of amphibian muscle and the presence of lactic acid; this appeared anaerobically and disappeared in the presence of oxygen. This showed that changes occurring in small molecular substances could be fruitfully followed.

In 1910, A. V. Hill started his researches upon the heat production of muscle, which he integrated with the observations of Fletcher and Hopkins upon lactic acid. Further support for the connexion between lactic acid and heat production in contraction was provided by R. A. Peters, working under Hill's direction, in 1913 by the demonstration that the heat produced in frog muscles, stimulated by their nerves, quantitatively reduces the heat formed in chloroform rigor. Quite early Hill took up the position that there was not enough heat found during the recovery process in oxygen to account for the total combustion of all the lactic acid which appeared, and that about four-fifths must be restored in some way to the muscle. On Hill's view, a current idea that lactic acid was burnt during the recovery was not consistent with the smallness of the heat produced. Unfortunately all the facts then known about oxygen uptake could not be marshalled to fit this view that something like the recharging of an accumulator went on during recovery, as will be seen by referring to the Croonian lecture by Fletcher and Hopkins (1916). Meyerhof entered this field with a much improved micro method of estimating lactic acid; this made rapid estimation possible as compared with the older methods which took about a week; also the microscale made it possible to use one frog limb as a control for phenomena studied in the other. (The older work required groups of ten pairs of frog limbs to get an accurate result.) With this microtechnique Meyerhof (1920) demonstrated experimentally the restitution as carbohydrate of over two-thirds of the lactic acid formed in activity; the energy for this was clearly supplied by the degradation of the remainder. Hence Hill's conclusion received a direct confirmation. So important was this result that Hopkins asked Miss D. L. Foster and Miss D. M. Moyle (now Mrs Needham) to repeat the work with the older technique of estimation in which the lactate was isolated as the Zn salt; they confirmed Meyerhof's results in detail. Thus was established the evidence upon which the original conception of the lactic acid cycle was based and the principles for the work upon what became known as the Pasteur–Meyerhof reaction. This fully justified the award of the Nobel Prize to Hill and Meyerhof conjointly.

Then followed much work that was necessary, but of a less exciting character. Much of it consisted of a more detailed repetition of earlier observations in Cambridge upon the relation between lactic acid formation and heat production, the so-called caloric quotient and of successive revisions of thermal balance sheets: in the main this was confirmatory. In addition there was work upon the caloric values for the combustion of lactic acid and of glycogen, important constants for the drawing up of balance sheets for the relation between lactic acid and the heat known to be produced in different phases of muscular activity from the increasingly accurate measurements of A. V. Hill and his colleagues. In 1925, there is a paper on the time course of contraction and lactic acid appearance in muscle, in which K. Lohmann appears for the first time as co-author; as a colleague of Meyerhof, Lohmann exerted much influence on the developments in this field. All this work arose from the desire to place upon a firm basis the 'lactic acid' theory of contraction, according to which muscular contraction was due to the effect of the liberation of lactic acid upon the colloidal apparatus; relaxation following upon its diffusion or removal from the site of action.

It must be realized that though this theory is now discarded, it did form the stimulus for the collection of many facts of importance in muscle; theories often pass but facts remain. The very concentration upon proof of the theory may prevent the investigator from paying due attention to facts which for the moment do not appear to fit, but which he feels can ultimately be explained. From 1925 onwards uncomfortable facts were appearing. Embden had done much work on the chemical nature of 'lactacidogen'. Now at Frankfurt Embden and his colleagues drew attention to the fact that when muscles were stimulated and *immediately* plunged into liquid air, part of the lactic acid production took place *after* the initiation of the contraction. It was contended with some vigour and cause that in the Embden experiments stimulation of the muscles was so drastic as to be unphysiological. Nevertheless Embden was right in principle, and the lactic acid theory as a description of the causal event in muscular action was nearing the end of its day. The finding that there was an unstable phospho compound in muscle, phosphocreatine by Fiske and SubbaRow and by Eggleton and Eggleton in 1927, shook confidence in the simple picture that only carbohydrate, lactacidogen and lactic acid were involved. Finally, when Einaar Lundsgaard of Copenhagen realized that contractions of muscle poisoned with iodoacetate took place without formation of lactic acid, the lactic acid theory became untenable.*

In the meanwhile other work destined again to be of the greatest significance was in progress. In 1926, two papers appeared, 'Uber die enzymatische Milchsäurebildung in Muskelextrakt', followed by two more in 1927; these became the experimental foundation for the development of the Embden-Meyerhof theory of glycolysis. Up to that time experimental work had appeared to show that the architecture of the muscle was somehow necessary for the glycolytic change, and that where activity had appeared in extracts it was due to bacterial action. In these papers it was shown how a simple cold KCl extract filtered through muslin contained all the enzymes necessary for a glycolysis from glycogen, and hexosephosphates, and that the extract would also work with glucose if a special activator known as hexokinase was also present. The point was that these changes occurred within 1 to 2 hours of making the extract, that is at a time when bacterial growth could not be influencing the results. Though the extracts were not free from solid matter, they were cell free. With this technique, there became possible the developments in Meyerhof's school such as the delineation of the role of adenosinetriphosphate by K. Lohmann, and the researches elsewhere not only of Embden but also of Parnas and colleagues in Lwow, of D. Needham and colleagues in Cambridge together with the fascinating extensions made by the Coris. All made contributions to the glycolysis scheme, and Parnas contributed some of the main equations. In Heidelberg there came by and with R. W. Gerard the extensions to nerve of the observation upon oxygen uptake in muscle.

After the finding of 'alactic acid' contraction, Lundsgaard, with characteristic generosity, went to work out his discovery in Meyerhof's laboratory; and what an interesting place it must then have been! In 1928 to 1930 there appear in the publications the added names of Meyer, Lipmann, Blaschko, Nachmansohn, Dean Burk, P. Rothschild, Schulz, Schmitt, Iwasaki, Ochoa, McCullagh, Boyland, Grollman and a little later Laser, H. Lehmann, Mohle, Gemmill, von Muralt, Jacobsen, Redenz, Brookens and Lwoff. H. H. Weber also owed much inspiration to Meyerhof. There are to be noted over this period researches upon crustacean phosphagen, viz. argininephosphoric acid (discovered by Meyerhof); upon the coferments involved; upon the isolation and synthesis of phosphopyruvic acid (in which Kiessling took part) and of other phospho esters related to these stages of glycolysis. In a way it is a rough truth to say that Embden's intuition produced the skeleton of the scheme, but that Meyerhof made it scientific fact. In addition to their contributions to the Embden-Meyerhof scheme of glycolysis, it is indeed remarkable and a tribute to the master to note how many of these men have made their mark in later years. To this period also belongs the discarding of the idea that methyl glyoxal was an intermediate in glycolysis. There were initially also unsuccessful attempts to instal 'phosphagen' in the place of lactic acid as the contractile substance; it is interesting to note that even at the present day the idea that a change in adenosinetriphosphate is the first event in contraction cannot be said to have been placed in a secure position.

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* It has been said that Lundsgaard's observations were a rediscovery; this is one of those half truths which are more false than true. The facts are that Pohl in 1887 had discovered that bromoacetic acid was toxic to frogs and had made a few observations upon the similar effect of iodoacetic acid. In the interval some others had observed what they thought was an increase of lactic acid in the presence of bromoacetic acid. In 1924-25, A. Schwartz and A. Oschmann observed that the contracture occurring in the presence of bromoacetic acid took place without increase of lactic acid. It was Lundsgaard who first established using iodoacetate that ordinary contraction occurred without appearance of lactic acid and realized its importance.

As already said, Meyerhof never lost sight of the importance of connecting the chemical and physiological events, and this may be instanced by work at this time with Hartmann upon muscle volume changes and with von Muralt on light absorption. Nevertheless it is fair to say that in fact the greater part of the work over this period consisted in the purely biochemical task of defining the intermediate stages in the glycolytic breakdown, this definition extending to the equilibria involved and also to their thermodynamics. An inspection of the number of papers of first rate importance appearing over these comparatively few years up to 1937 gives some idea of the efficiency of the laboratory as well as of the competence of its director. Judged by any standards the achievement is remarkable. It is somewhat invidious to select; as previously mentioned, of cardinal importance was the definition of adenosinetriphosphate by Lohmann, giving added significance to the earlier discovery by Embden and Zimmerman, 1927, of muscle adenylic acid and to Meyerhof's own on the existence of pyrophosphate in muscle. Each year the significance of this compound seems to become greater in biochemistry, serving as it does as a central store of readily available phospho groups. The important stages of the phosphoglyceric acids and of phosphopyruvic acid were also defined and the function of enolase. The work on muscle was also integrated with that on yeast to demonstrate the fundamental similarity in all but the late stages of carbohydrate breakdown. The economy of nature in its use of biochemical reactions seems to have been one of Meyerhof's conceptions; this certainly applies to fermentation in yeast where it may be said that only the final stages are different from those of muscle. In this connexion we may note the work by Ohlmeyer upon cozymase in muscle, and the elucidation by Lohmann and Schuster of cocarboxylase as vitamin B_1 pyrophosphate, the active form of vitamin B_1 (aneurin thiamine); this was most important because it connected pyruvic acid metabolism in yeast with work in Oxford by Peters and his associates, who had shown that thiamine is concerned with the metabolism of pyruvic acid in animal tissues. In 1937, three new workers appear, P. Ohlmeyer and S. Ochoa and M. Dubuisson. All this work, and the delineation of the Embden–Meyerhof scheme in its main form as we know it today, were summarized by Meyerhof in 1937 in his review on the intermediary changes of enzymatic degradation of carbohydrate and again in an article in *Nature*, 1937, which is the report of a lecture. With the exception of the one piece of work on thiamine pyrophosphate, the Meyerhof school were not concerned with the subsequent oxidation of the intermediate pyruvic acid; all work was concerned with the phospho changes progressing before this was reached.

To the period in question also belongs the discovery contemporaneously and independently by D. M. Needham and Pillai and by Meyerhof of the phosphorylation coupled with oxidation; this was the direct precursor of Lipmann's conception of high energy phosphate, and of the modern researches by Kalckar, Belitser and Tsibakowa, Ochoa and the Coris upon phosphorylation occurring during oxidation, now known as 'oxidative phosphorylation'. Meyerhof was also early in the isotope field by using radioactive phosphate as a means of proving the reversibility of the stages of glycolysis, a necessary completion of his early work on the synthesis of glycogen from lactic acid by surviving muscle tissue. The only stage which he could not find to be reversible *iu vitro,** that of the synthesis of phosphopyruvic acid from pyruvate and adenosinetriphosphate, was later proved by Lardy to require the presence of K ions.

By now in 1938 Meyerhof's period as leader of the Heidelberg Institute was drawing to a close and he found it necessary to flee to France and then to the U.S.A. Though deprived of the help of his pupils (and particularly of his assistant Schulz) and of his organized laboratory his researches continued; for instance he had never been happy with the experimental proof of the existence of a non-phosphorylating glycolysis; he now showed that by the correct choice of experimental conditions phosphorylation did occur during glycolysis in embryo extracts; this proof was needed to clear up an awkward discrepancy. When finally he got to Philadelphia his output started again and steadily increased until the time of his death. Even at the end we find papers upon the heat of hydrolysis of acetyl phosphate and of metaphosphate; this parallel thought about the biochemistry and thermodynamics was so characteristic of him that we find him devoting a long session of his later years in New York (New York Academy of Sciences) to attempts to summarize the thermodynamics of muscle chemistry. Pitfalls always await the biologist in this field, and the author knows that M. Dixon had a correspondence with him over some controversial points, and there do seem to have been errors; but it was one of those cases where the errors balanced; this as mentioned by Harden even occurred to Lavoisier once when he was calculating a balance sheet for fermentation by yeast! During this later period of Meyerhof's life, though nothing emerged as sensational as in his earlier work, much of significance was done, and we may yet have to wait for the harvest. With Kielley for instance he found a new enzyme breaking down ATP and succeeded in tidying up several points in the glycolytic scheme. He also took up the problem of transfer of phosphate groups by acid and alkaline phosphatases.

The researches briefly outlined leave no doubt that Meyerhof was a master in the field of physiological chemistry, an important enzymologist, who must be regarded as one of the leading pioneers of his day. It is evident that he formed a constant source of inspiration to his colleagues who remember him with the greatest affection. The most impressive evidence of this is the volume dedicated to him on his 65th birthday by old friends and colleagues. It is tragic that the work at Heidelberg should have been cut short; at the same time the fact that his spirit could rise above his environmental difficulties should always remain an inspiration to other scientists; and it may well be that his presence in the U.S.A. in his closing years has a significance which we cannot yet appreciate.

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* The stage hexosemonophosphate + ATP→hexosediphosphate + ADP has not yet been shown to be reversible *in vitro*.

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R. A. Peters

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J. H. Orton

JAMES HERBERT ORTON

1884-1953

James Herbert Orton was born on 11 March 1884 at Bradford, Yorkshire, his father being George William Orton of Westmorland who married Sarah Rebacca West of farmer stock from Lincolnshire. James was the fourth child in a family of six brothers and six sisters, four of whom died as infants, one at the age of 12 and another at 27.

He was town bred, but at an early age he became interested in the country and would spend whole Sundays on long walking expeditions hunting for frogs and newts, and birds nests. His childhood days were healthy and happy though owing to early financial troubles his parents had little means. At the age of 11 years, while attending part time at White Abbey Board School in Bradford, he was employed half-time as an errand boy. He himself has remarked that he thereby attained a certain degree of independence, a character for which he was noteworthy throughout his life. He enjoyed his school work and remembered especially his appreciation of one teacher who taught physiology and rugby football.

When 13 years of age he became apprenticed as a mechanical dentist, serving his time until he was 21. During those years he attended evening classes at the Technical School and later took his London University Matriculation examination.

In 1906 he won a National Scholarship in Biology and proceeded to the Royal College of Science, London, where he became a 1st class Associate in Zoology in 1909.

All his friends will remember Orton as an indefatigable worker; a contemporary of his student days, Dr W. Rushton, remembers him frequently sitting on the doorstep waiting for the college doors to open and being the last to leave. He was always very active physically; he played centre forward for the Royal College of Science 1st Association Football team from 1906-8. Orton kept this love of outdoor exercise and games to later life, playing for the Plymouth 1st hockey team from 1924-28 and the Plymouth 2nd cricket eleven from 1920-29. Those who can remember Orton in his Plymouth days will not forget seeing the way he strode across the Hoe to his work.

In 1909 he was elected Marshall Scholar of the Royal College of Science and he graduated B.Sc. London with 1st class honours in 1910. In that year he was appointed to the staff of the Plymouth laboratory of the Marine Biological Association of the United Kingdom as Assistant Naturalist. Here, except for the first war years, he remained until 1929 on his appointment to the Professorship of Zoology at Liverpool University in succession to W. J. Dakin. It was while he was at Plymouth that Orton did the greater part of his research.

In 1939, while still Professor of Zoology, he became Director of the Marine Biological laboratory at Port Erin, in the Isle of Man, the administration of which had been transferred from the Liverpool Marine Biological Committee to the University in 1923. From then until 1939 it had formed part of the Department of Oceanography which was founded and endowed by Sir William Herdman. Orton was again away on service during the second world war, but on his return to Liverpool in 1944 he initiated the beginning of an increase in staff and expansion of the scientific programme of the Port Erin laboratory which is still bearing its fruit. He retired from the Professorship in 1949 on reaching the age of 65.

Orton received the Huxley Medal from the Royal College of Science in 1914 for his researches at Plymouth, and he was awarded his D.Sc. in the same year. He was elected F.R.S. in 1948. On his retirement from the University he was made an Emeritus Professor.

Orton served in both world wars. In the first he rose to the rank of Captain, being Assistant Commandant at the Southern Command Royal Garrison Artillery Signalling Training Depot. In the second he served first in the Royal Artillery, later transferring to operational research with Coastal Command.

Apart from the influence of his research Orton played a large part in the development of the study of marine biology by his stimulating teaching. He once said to me that he had taken up his Univeristy work too late after so many years in whole-time research, yet there is plenty of evidence of his success as a professor. It is only necessary to remark on how many of his former pupils at Liverpool are now on the staffs of different marine biological laboratories to realize his influence.

But even before he went to Liverpool he had made his impression on many young zoologists, for in 1914 he took charge of the Easter Courses held at the Plymouth Laboratory which had been started originally by Garstang. Apart from the war years he supervised these courses until 1929, and later he took courses in September. Orton's keen and eager participation at these vacation gatherings must remain a vivid memory to many alive today, for all together some 450 or more students passed through his hands, of whom at least 70 remained in the zoological field, 12 became Professors, and 8 Directors of Marine Laboratories or equivalent organizations. I was myself one of those fortunate enough to attend, and in that one class in 1921 the following also took part: the late Professor K. N. Bahl, Professor L. E. S. Eastham, Dr Honor B. Fell, Miss S. Garstang (Mrs A. C. Hardy), Dr C. C. Hentschel, Dr L. Harrison Matthews, the late C. C. A. Munro, Dr C. F. A. Pantin, W. H. Potts, Dr H. Sandon, H. P. Sherwood, R. H. Teale, and Dr E. Trewavas. It should be remembered that this was at a period when the numbers of advanced students taking zoology at Universities was still small and when a general expansion of science was taking place. The number of course in marine biology was then not as great as now, so that a much higher proportion of zoology students came to Plymouth. This probably accounts for why so many of those who passed through Orton's courses continued in a zoological career. While at Liverpool he reorganized the students' courses at Port Erin where his vivifying influence was soon felt.

In 1922 Orton also delivered a series of lectures in Marine Bionomics at London University, and in 1935 he was Frank Buckland Professor, choosing as his subject 'Oyster biology and oyster culture'.

While at the Royal College of Science Professor Dendy gave him a collection of radulae of the slipper limpet, *Crepidula fornicata*, collected at West Mersea, suggesting that a comparison should be made of the radula in American and English stocks of this oyster pest which had been introduced into this country in 1880. Orton remarks, 'I took the matter up, but after reading Professor Conklin's work, on the genus *Crepidula*, decided to extend the investigation.'

From his observations Conklin had suggested that it might be that protandrous hermaphroditism occurred in *Crepidula*. Orton showed definitely that this was so. Having indicated that the chains were permanent he made a detailed study of all individuals in a large number of chains. He was able to show that the largest and lowest members of a chain were always female and the smallest ones at the top always male. He also found many intermediate stages in the middle proving the change over of the sex from male to female. A careful examination of these latter specimens showed that the development of the primary sexual characters was always in advance of that of the secondary sexual characters.

Orton extended these observations to *Calyptraea chinensis* and showed that it also was a protandrous hermaphrodite.

It seems likely that this introduction to mollusc biology and especially its association with oysters may have influenced Orton's future work.

On arriving at Plymouth Orton at first continued his studies on *Crepidula*. 'From my studies of the habits and anatomy of this sluggish animal I had formed a hazy idea that, since the gut is very strongly ciliated throughout, food was probably drawn in at the mouth in a current of water. As a result of this idea, I concluded that the radula in later life was an obsolete organ which the animal possessed merely as a heritage from its ancestors. On my expressing this opinion to Dr Allen, he pointed out that if *Crepidula* possessed an obsolete but well-developed radula, then the phenomenon appeared to be a new one, which required to be carefully investigated.'

Examination showed that *Crepidula* was undoubtedly a ciliary feeder but that the radula was also used to grasp the food as it passed towards the mouth. Its function had become that of grasping rather than rasping. This discovery caused Orton to extend his observations to other molluscs. He found that a number of gastropods, including *Calyptraea* and *Capulus*, had similar feeding mechanisms. He compared these with lamellibranchs for which it was already known that the gills were essentially organs of feeding. He was a pioneer in the analysis of the ciliary mechanism in lamellibranchs showing that the main food and respiratory current is caused by the beating of the lateral cilia, and that the collection and transport of food material is done mainly by the frontal cilia, assisted by the latero-frontal cilia when they are present. This work of Orton's was the most detailed that had been done about that time apart from the work of Kellogg, and of Wallengren in 1905, which was apparently unknown both to Orton and Kellogg. The work on gastropods was certainly the first of its kind.

Similar investigations were now extended to other groups including polychaetes, *Solenomya*, tunicates, brachiopods, *Phoronis* and *Amphioxus* and Ammocoete larvae. In this survey Orton brought to notice the evolutionary convergence in ciliary feeding mechanisms in widely separated groups of the animal kingdom. All had the differentiation into lateral and frontal cilia, the lateral being the main current producers and the frontal driving the food towards the mouth. The description of feeding in *Amphioxus* was the most detailed hitherto and showed the passage of mucus from the endostyle to the dorsal wall of the pharynx, and the selection of food size by means of buccal tentacles. The work on brachiopods was the first of its kind.

Then followed a brief paper on the Plymouth species of *Cucumaria* showing Orton's general faunistic interest.

Meanwhile, already in 1914, he had started collecting material for a general study of growth in marine invertebrates. Orton's stated aims were:

1. To establish the age of common marine invertebrates.
2. To determine the minimum age at which these forms begin to breed.
3. To examine the rate of growth at different seasons of the year and under different conditions.
4. To investigate the fecundity of different forms so far as possible.
5. To collect the scattered literature on these subjects.

His general methods of obtaining the necessary data were by the immersion of various objects in the sea at a known time and subsequent observation and collection of the various kinds of animals growing on them, and by entrapping young and larval free-living animals and observing their rate of growth.

The results were published in 1914 in a paper entitled 'Preliminary account of a contribution to an evaluation of the sea', and provided a wealth of information on the life histories of sessile marine animals in many invertebrate groups. It was shown that many of these animals were annuals, and that some matured in from 1½ to 2 months and could pass through two or three generations in a year.

The first great war now intervened and there was to be a gap of several years before the next publication in 1920. This was entitled 'Sea-temperature, breeding and distribution of marine animals'. This was largely a review of the literature on this subject reinforcing the general conclusions of the influence of temperature which Orton had himself realized from the results of his observations on growth of marine animals.

In October 1920 Orton was called to attend a meeting of representatives of the Development Commission, the Ministry of Agriculture & Fisheries, and the Oyster Merchants and Planters Association to discuss an examination of the cause of abnormal mortality which had occurred among oysters in the oyster beds of the Thames Estuary during the summer of 1920. It was decided that investigations should be begun under Orton's general direction. In this work he was to have the assistance of Dr O. L. Brady on the chemistry of explosives and of Professor J. Eyre on bacteriology. It was popularly held that the dumping of explosives, including T.N.T., in certain areas of the Thames Estuary after the war was the cause of the mortality.

Orton undertook this work with characteristic vigour and breadth of vision. From the point of view of providing an answer to the problem it was disappointing. As has happened since with *Zostera*, the peak of the mortality was essentially passed before a decision to investigate was made. The real cause could not be found, but it was at any rate shown that it was unlikely that T.N.T. had been responsible.

Throughout the large report produced as a result of the investigation Orton stressed our lack of knowledge of the normal state and general physiology of the oyster. He examined the problem from the point of view of weather and general environmental conditions, poisons, parasites and pests and made many detailed and fundamental observations on the oysters themselves, including original data on myolysis and bleeding. As a result he made a number of recommendations for future fundamental research, and on reading these recommendations it can be seen how he himself bore them in mind and allowed them to shape his own research in later years. One of the immediate outcomes of his recommendations was the appointment of C. M. Yonge to the staff of the Plymouth laboratory for two years to carry out a special investigation on the feeding and the physiology of digestion in the oyster.

In June 1924 he attended a meeting called by the Cornwall Sea Fisheries Committee between the Falmouth and Truro authorities and the oyster dredgermen to discuss the question of the need for an increased measure of protection for the oyster beds in the Fal Estuary. In September of the same year the Ministry of Agriculture & Fisheries convened an enquiry meeting at Falmouth into the decline of the oyster fishery. Orton attended as technical adviser and the outcome was that he undertook a survey to estimate the stock, the rate of fishing and the distribution of the stock. This survey resulted in the uncovering of a number of research problems which Orton himself later investigated. Among these was a study of the so-called 'dumpy' oysters which formed a large proportion of the Fal Estuary stock. It was concluded that they formed a valuable reserve for purposes of reproduction. In this, however, he ignored the possibility that the poor shape of the oyster might be inherited and the shell standard of the beds not improved.

One outcome of his study of the fertility of normal and dumpy oysters was the disclosure of lunar periodicity in breeding, a subject which he followed up in some detail.

Another line he followed up was the study of fattening of oysters and their shell growth. He showed that oysters in the Fal Estuary had two periods of shell growth in the year, one in the spring and the other in the autumn. These occurred in the pre-breeding and post-breeding part of the season. He found similar periods of shell growth in beds in the River Blackwater, Essex. He linked these periods with the temperature of the environment, concluding that the higher temperatures led to development for reproduction, a state that is on the whole antagonistic to shell-growth. Good fattening occurs when food supply is abundant and neither shell-growth nor breeding are taking place. Orton at that time suggested manuring of oyster beds to secure increased fattening.

It will have been realized that very early in his career Orton became interested in the sex phenomena in molluscs, and this culminated in a study of Fal oysters in his notable publications on sex change in oysters. Sex reversal had been suggested by earlier workers, but until Orton undertook his comprehensive investigations it lacked convincing proof. By exhaustive examination of great numbers of specimens he showed how rapid was the change of sex. 'In from 2 to 3 months after egg-spawning sperm-development is completed and the male phase begins to wane, and a small percentage of individuals may become female or actively female-functioning again. In 12 months after the last egg-spawning a significant number of individuals become once more functional females.'

Of this work Dr H. A. Cole, Director of the Ministry of Agriculture & Fisheries Shellfish Research, says: 'His series of papers entitled "Observations and experiments on sex-change in the European oyster" were very thorough and comprehensive, and practically all his conclusions have since been confirmed. Moreover, his work led to a whole series of studies on the breeding of oysters in practically every country in the world in which they are cultivated. This series of papers appeared over a long period, the final one being that published in the *Mém. Mus. Hist. Nat. Belg.* in 1936 which I regard as a very fine piece of work—I think his best.'

As regards the work on shell growth in native and Portuguese oysters Dr Cole says: 'He laid the basis of future studies, studies which we are now taking up again and which will be, we think, of great importance in selecting and propagating quick-growing, early maturing strains of oysters. Although Orton never discussed with me the desirability of genetical selection of improved races of oysters, his attitude was that we should find out as much as possible about conditions of growth in order that well-grown and fat oysters could be produced at will.'

On other aspects of Orton's work Dr Cole says: 'He appreciated the dangers inherent in the spreading of the pest *Crepidula* in British oyster beds. Unfortunately his warnings were not heeded and no serious work was done on slipper limpets until quite recently. A small but quite important series of papers dealt with the Oyster Drills, *Urosalpinx* and *Ocinebra*, and he was first to recognize *Urosalpinx* and to observe that severe winters result in very severe selective mortality of *Ocinebra* in Essex. Indeed the winters of 1940 and 1947 appear to have exterminated *Ocinebra* on this coast.'

In 1919 he started an experiment (in continuation of his work on the evaluation of the sea) to obtain information on the rate of growth of the common cockle, *Cardium edule*. For this he had an experimental cage sited in the Yealm Estuary and received the close assistance of Mr W. H. Searle ('Bill') the veteran fisherman collector of the Plymouth laboratory. In this investigation he elucidated the winter growth rings, and the 'disturbance' rings, which are normally less deep and result from the stoppage of shell-growth for a few days due to some disturbance of the animal. He showed the presence of similar rings in the common mussel, *Mytilus edulis*. At the same time he established the breeding period of the cockle.

Orton extended his observations on growth and breeding to other molluscs, notably the limpet and the mussel. He also made a great number of lesser observations on a wide range of marine animals, one of the more interesting being on the feeding of the hermit crab, *Eupagurus bernhardus*. He found that at certain times of year at any rate the crab fed mainly on micro-organisms using its third maxillipede for the purpose. He also recorded ciliary feeding in *Aurelia*.

There is one other field in which Orton was among the earlier pioneers; this is the biological study of the effects of anti-fouling compositions. During the course of his early observations on the rate of growth and breeding of marine organisms he realized that the knowledge thus obtained would supply invaluable material for experimental studies on the properties and efficacies of anti-fouling paints used to prevent organisms from settling and growing on the bottoms of ships and on other underwater structures. He began this work in 1919, although the results were not published until 1929. By exposing materials in the sea for periods varying from 14 months to 3½ years he tested the power of a number of paints and other substances for inhibiting growth of marine organisms, their preservative value and their durability in sea water. Although much research has been done since that time on problems of anti-fouling there is no doubt that Orton's work formed a valuable contribution to the foundation literature on the subject.

All the above research was done while Orton was at Plymouth. When he went to Liverpool he continued to publish results of observations made while at Plymouth, but his own output naturally decreased considerably. Thereafter his energies were transferred to the encouragement of his research students and the development of the Port Erin laboratory.

Such time as he could himself find for research was spent chiefly on continuation of his studies on *Patella*. Dr A. J. Southward writes on this: 'As an outcome of his work on growth and breeding of marine animals he became interested in the common limpet. At first obtaining information from the settlement on a new wharf, and on test panels and cages, he soon turned to the normal rocky shore habitat, and was able to show, on circumstantial evidence, that the limpet, like many other Mollusca, underwent a sex-change. The increasing complexity of the limpet investigation and his transfer to Liverpool resulted in a temporary cessation of the work. However, before this he had made significant contributions to intertidal zonation, being the first to stress the importance of wetting versus desiccation and the role of sunlight and wave action in altering the upper limit of intertidal organisms.

'The establishment by Fischer-Piette, that the so-called varieties of *Patella* (one of which was unknown to Orton, and the other regarded as a habitat variety) were separated by apparently specific differences stimulated him to take up his limpet researches again at the end of the war. By examining large numbers of limpets from all over Britain, he was able in 1946, to confirm his earlier work, and show that the sex-change was a widespread phenomenon. These results are more convincing than those obtained formerly, and while providing virtual proof of sex-change from male to female in a section of the male population, point also to the probable occurrence of two types of males in *P. vulgata*. Simultaneously with Fischer-Piette he showed that the species of *Patella* were indeed separate, since the breeding periods were different. Rapidly he built up a team of workers interested in various aspects of limpet biology and a vast amount of information was collected, including many observations on the importance of *Patella* in controlling the algal growth on the shore. Orton had always been interested in spawning stimuli, but even his vigorous experimental attack failed to show a definite cause of spawning in *Patella*. For one species, the evidence collated pointed to a combination of falling sea-temperature, neap tides, and wave-action (physical shock). He was beginning the writing up of this work, together with a report on pollution in the River Dee, when he had his first serious attack of coronary thrombosis in 1950. His recovery was slow, but though he was able to complete the pollution work, the limpet material had to be set aside for a while. However, he continued his observations at Plymouth and Trevone, and was also able to take part in a study of the distribution of animals and plants on the breakwater in relation to wave-action, a long-projected investigation. It was somehow typical of the man that he should be engaged on the revision of a paper on this shortly before he was taken seriously ill for the last time, and that he should have examined a sample of limpets only a few days before his death.'

Mr J. M. Dodd says of the limpet research: 'In many ways this work typifies his methods and thoroughness better than any other. He succeeded in interesting many people in it and in getting their co-operation, but the ideas were his and most of the hard work was done by him. The work was undoubtedly bearing fruit at the time of his death. He was able to forecast spawning in *Patella vulgata* at St Andrews with great accuracy in October 1952.'

If these results should remain unpublished it will be a real loss to marine biologists and it is much to be hoped that some means will be found, if possible, of getting his results together.

During his last years Orton also studied sex phenomena in the salmon, and his research with J. W. Jones showed conclusively that the salmon parr matured before leaving the river for the sea and played their part in fertilizing the eggs on the redds.

Apart from his development of marine research at Liverpool and Port Erin he also built up a school of fresh water biology and pollution research, and was largely concerned in the initial stages of planning the laboratory and work on Lake Bala.

In reviewing Orton's work in general the most remarkable feature is the number of biological lines of research in which he pioneered. He kept an interest in all these lines and throughout his life ensured their further development by arousing the interests of students.

Orton had a very independent nature and his heart and soul were in the development of research. In the administrative realm he was at times too forthright and lacking in diplomacy, yet all who knew him felt his charm and enthusiasm when he turned to subjects of biology nearer to his heart.

He had a great knowledge of marine animals and was essentially a field naturalist and recorder of facts. Indeed, his insistence on the introduction of as many possibly relevant data as he could into his published papers make them often difficult to read. Everything that he had recorded and which might eventually be found to have some bearing on the subject had to be included. This insistence led to the continual addition of more and more qualifications to manuscripts already completed, so that often they became the terror of the compositor! I anticipate, however, that in years to come people will turn more and more to his papers for records of environmental conditions existing during the years in which he worked.

In his University work Orton was better at developing potential research students than routine teaching. Those most fitted for research careers were probably spotted by him at Easter courses where his demonstrating brought him into closer contact with individuals.

All, and they are many, who have been fortunate enough to have had their lives influenced by such contact remember Orton with affection. His boyish spirits when once near the shore, whether at Plymouth, Port Erin, or the Mersey, were unforgettable, and his keen, eager face right up to near the end belied his age. He died on 2 February 1953.

J. H. Orton was twice married, first in 1911 to Emma Teresa Ackroyd who died in 1941, and, secondly to Haidee Lodge in 1942 who survives him. He had one son by his first wife, Peter Darbishire Orton, M.A., A.R.C.M., who is assistant music master at Epsom College but has inherited his father's love for natural history.

F. S. Russell

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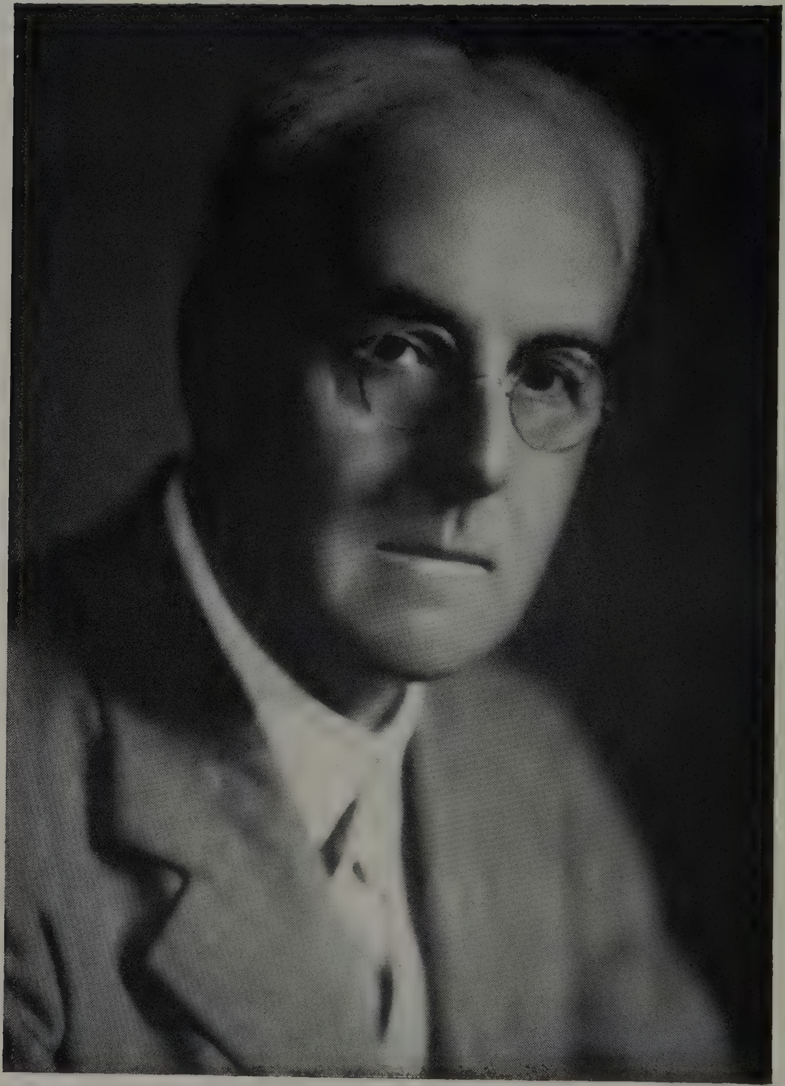

L. F. Richardson

LEWIS FRY RICHARDSON

1881-1953

Lewis Fry Richardson, D.Sc., F.R.S., who died quietly in his sleep, at Kilmun, Argyll, on 30 September 1953, had been a Fellow of the Society for 27 years and had contributed many papers to the *Transactions* and *Proceedings*.

Richardson, born at Newcastle on Tyne on 11 October 1881, was the youngest child of David and Catherine Richardson who had a family of five boys and two girls. David Richardson was a tanner: the Richardsons had, in fact, been tanners for three hundred years. The story of the family is given in a book by A. O. Boyce, published in 1889 *Records of a Quaker Family; the Richardsons of Cleveland.* Contemporary relatives of note were John Wigham Richardson, shipbuilder, a first cousin of David, and Henry Richardson Procter, F.R.S., Professor of Tanning at Leeds, a second cousin once removed (a third cousin of Lewis). Hugh Richardson, educationist and geographer, was Lewis's elder brother; Sir Ralph Richardson, actor, his nephew.

David Richardson's mother, Sarah (*née* Balkwill) was the daughter of a pharmaceutical chemist in Plymouth and in her youth she got the nick-name 'Sal volatile' because she was gay. (As his father and grandfather both went south for their wives, it is, perhaps, not to be wondered at that Lewis did the same.)

Catherine Richardson (*née* Fry) came of a family of corn merchants in Devon. She was a second cousin of David, their common great-grandparents being Joseph and Susanna (*née* Browne) Hancock.

'My father', records Richardson, 'was affectionate and constructive. His arguments were confused. My mother, on the contrary, was unemotional, clear-minded, and critical. I seem to have a bit of both.' He adds, to a paragraph about relatives and ancestors, two of his characteristic critical comments, 'It is difficult to say who were noteworthy as some of the best work passes unpraised.' 'Galton's method of studying heredity by searching in biographies does not properly distinguish between social tradition and biological transmission.'

Richardson married, on 9 January 1909, Dorothy, younger daughter of William Garnett, M.A., D.C.L., who had been Clerk Maxwell's demonstrator at the Cavendish Laboratory and was then Educational Adviser to the London County Council; Garnett College was (in 1953) named after him. He had three sons, all achieving distinction at Cambridge both in the triposes and on the river. The two younger, Stuart and Kenneth, were killed in the first world war. The eldest, J. C. Maxwell Garnett is best known for his work in organizing the League of Nations Union and as Principal of the Manchester College of Technology.

To their deep disappointment, Lewis and Dorothy Richardson had no offspring. They therefore adopted, in succession, two sons and a daughter: Olaf K. M. Richardson, who became a motor engineer; Stephen A. Richardson who, after eight years in merchant ships becoming Chief Officer and obtaining his master mariner's certificate, went to Harvard and studied psychology and human relations prior to taking an appointment on the staff of Cornell University; Elaine D. Richardson who, after studying dramatic art, married, in 1950, a meteorological officer, M. F. Traylen. All three made good and formed a real family, united in affection, respect and esteem for their head. I mention this primarily as illustrating Richardson's determination to make the fullest contribution within his power to the Good Life of mankind and incidentally as an indication of the effect of environment on development.

After a period at Newcastle Preparatory School, where his chief enjoyment was Euclid 'as taught by Mr Wilkinson', Richardson entered Bootham School, York in 1894. There he came under the influence of J. Edmund Clark who for many years prepared the annual Phenological Report of the Royal Meteorological Society. 'Clark gave us glimpses of the marvels of science.' His successor, Richardson's eighteen year older brother Hugh 'taught us how to observe and describe, while he supplied us with very little information (the Heuristic Method of Armstrong).' 'Another master A. Neave Brayshaw (who later wrote a history of Quakerism) left me with the conviction that science ought to be subordinate to morals.'

In 1898 Richardson left Bootham, with a Natural History Exhibition and a Leaving Scholarship and spent two years at the Durham College of Science (Newcastle) 'acquiring information and techniques' in mathematics, physics, chemistry, botany and zoology. He was second in the examination, 'Nora Aldis being first.' He observes, 'the social and ethical discipline of Bootham Schools was lacking.'

In 1900 he entered King's College, Cambridge as a Minor Scholar (resigning the emolument at the end of his second year). There he 'continued to acquire information and skill from more brilliant teachers, notably in physics, from Professor J. J. Thomson and Dr G. F. C. Searle'. He took the Natural Science Tripos Part I in 1903 and got a 'First'. His formal education then ended but twenty years later he studied psychology as an external student of University College, London, and took the London Special B.Sc. examination in psychology, with second-class honours in 1929 (three years after he had been given the London D.Sc. in physics on his published researches).

It may be convenient, before giving an account of his work, to mention here in historical sequence the different appointments which Richardson held.

1903-4. Student Assistant at the National Physical Laboratory (Metallurgy Department).

1905-6. Junior Demonstrator in Physics, University College, Aberystwyth.
1906-7. Chemist to National Peat Industries Ltd, 'whose managing director stole a large sum and fled abroad'.
1907-9. Assistant at the National Physical Laboratory (Metrology Department).
1909-12. In charge of the chemical and physical laboratory of the Sunbeam Lamp Co., Gateshead (where he organized a troop of scouts).
1912-13. Demonstrator and Lecturer in Physics, Manchester College of Technology.
1913-16. Meteorological Office, Superintendent of Eskdalemuir Observatory.
1916-19. In the Friends' Ambulance Unit in a motor convoy (S.S.A.13) attached to the 16th French Infantry Division.
1919-20. Meteorological Office, nominally in charge of experiments in the computation of the sequence of weather by numerical processes. He worked at Benson, Oxfordshire, where W. H. Dines was in charge of 'Upper Air Investigation'. (Richardson revived the village troop of scouts.)
1920-29. In charge of the physics department at Westminster Training College.
1929-40. Principal of Paisley Technical College and School of Art.
1940-53. Retired to do research on wars. Research also in eddy-diffusion.

He was one of the two honorary secretaries of the Royal Meteorological Society 1921-4, member of the International Commission for the Investigation of the Upper Air 1921-30, member of the National Committee for Geodesy and Geophysics 1923-33, Fellow of the Royal Meteorological Society, the Physical Society, the Institute of Physics and the British Psychological Society.

Richardson's work may be regarded as falling, broadly, into four main divisions, (i) his development of the application of the method of finite differences to the solution of physical problems, including the major problem of meteorology viz., the computation of the physical state of the atmosphere for an epoch finitely subsequent to that for which the state is known by observation. (ii) His contribution to our knowledge of diffusion, especially to eddy-diffusion in the atmosphere. (iii) His development of the application of mathematics to the study of the relations between nations, especially to elucidate the effects, in combination, among a number of nations, of armaments, trade, communications, rivalry and grievances on the stability of the regime. (iv) His educational work at Westminster and Paisley.

As chemist to National Peat Industries, Richardson became so interested in the flow of water in peat that he made it the subject of a dissertation for a Fellowship at King's. He only just failed to be elected in the face of stiff competition. One is inclined to speculate on the course of his life if the managing director of the peat company had not defalcated—would Richardson perhaps have put Britain in the forefront of hydrological research? What actually happened was, that, having been brought face to face with the practical necessity of solving problems involving differential equations which were not formally soluble, he was led to study approximate methods. This resulted in his first major paper 'The approximate arithmetical solution by finite differences of physical problems involving differential equations.' The method depends for its success on the assumption that, if finite differences are 'centred', not only do the differential co-efficients of the function concerned involve only even powers of the difference-step, h, but so does the error of the approximate solution—or, in other words, the difference between the approximate solution and the exact solution involves only even powers of h. Thus if approximate solutions ϕ_1 and ϕ_2 are found for two sizes of the step, h_1 and h_2 and the values of h_1 and h_2 are small enough for the terms in h^4 and higher powers to be negligible, then a closer approximation, f, is given by

$$f = \frac{h_2^2\phi_1 - h_1^2\phi_2}{h_2^2 - h_1^2}$$

Richardson showed how the method could be applied to obtain approximate solutions of equations of the type $\frac{\partial^2\phi}{\partial x^2} + \frac{\partial^2\phi}{\partial y^2} = 0$ with given boundary conditions, the 'boundary' being the limit either of space or of time of the space-time continuum in which the equation holds. He distinguished between two types of problem which he called 'jury' and 'marching' problems according to the character of the boundary conditions. A 'jury' problem is one 'in which the integral must be determined with reference to the boundary as a whole'. A 'marching' problem is one 'in which the integral can be stepped out from a part of the boundary'. If there is only one independent variable, x, and the boundary condition is specified, (*a*) at both ends of the range of x, the problem is 'jury', (*b*) at only one end of the range of x, the problem is 'marching'. The example of stresses in a masonry dam, treated in this first paper is a 'jury' problem. The problem of weather prediction is a 'marching' problem.

J. A. Gaunt, stimulated by Richardson, showed that the initial step must be taken according to one of two fixed rules according to the initial boundary conditions. Only in this case does the formula for f fully apply. In fact the formula may give a result worse than either ϕ_1, or ϕ_2. It does, for example, in Richardson and Gaunt's simple case of finding the value of y for $x = 1$ which satisfies the equation $dy/dx = -y$ with the boundary condition $y = 1$ when $x = 0$ and $0{\cdot}1$, $0{\cdot}2$ as values of h_1, h_2 if the initial values adopted are both greater or both less by the same amount, c, than the initial values $0{\cdot}90551$, $0{\cdot}81820$ which would give zero errors for ϕ_1, ϕ_2 at $x = 1$. The errors of ϕ_1, ϕ_2 will be $-1{\cdot}17c$, $1{\cdot}51c$ and f, from the formula, will have an error $-2{\cdot}06c$. The formula gives a value with a negligible error only if the initial value for h_2 minus $0{\cdot}81820$ is $3{\cdot}1$ times $0{\cdot}90551$ minus the initial value for h_1. The initial values given by Gaunt's formulae, 0·90499, 0·81980 do satisfy this condition; and they give, for $x = 0{\cdot}2$, $0{\cdot}4$ etc., values of f correct to five significant figures, whereas initial values which give zero error at $x = 1$, give oscillating errors 0·00040, −0·00049, 0·00019, −0·00106. Even if the initial 'guess' does not satisfy the strict conditions necessary to secure the full improvement in accuracy, the method gives very small errors with reasonably small h; and it was a great achievement to introduce this procedure for dealing with practical problems of physics and engineering and securing a degree of accuracy far surpassing that previously obtainable.

Richardson returned in later years to this problem of approximate solution of partial differential equations. In 1950 he showed how the results of what he called the 'purification method for computing the latent columns of numerical matrices' could be utilized in the solution of equations such as $\frac{\partial^2 P}{\partial x^2} + \frac{\partial^2 P}{\partial y^2} = \lambda P$ with $P = 0$ at the boundary and $\frac{\partial^2 P}{\partial x^2} + \frac{\partial^2 P}{\partial y^2} = 0$ with P given at every point of the boundary.

Richardson regarded his early work in this field of application of finite differences, where he had used a closely similar purification method, as the progenitor of many later developments, including that of 'relaxation'. In this 1950 paper he comments 'I suppose—though this may be controversial—that "relaxation" is sufficiently alike to "purification" for the choice of purifiers to have some relevance to relaxation.' He was, I believe, pleased by the reference to his 'useful simple method' as one appropriate for use by workers in the field of differential equations who 'may wish to obtain more accurate solutions than the simple relaxation process can provide without becoming involved in finite difference theory' in L. Fox's 1950 paper on *The numerical solution of elliptic differential equations when the boundary conditions involve a derivative*; and by the inclusion of an application of his method to a 'jury' problem, the solution of $\mathrm{d}^2y/\mathrm{d}x^2 + 1/x.\mathrm{d}y/\mathrm{d}x + y = 0$ with boundary values specified at both ends of the range of x; approximate values were obtained to eight significant figures, the maximum error being 20 in the last two figures.

In 1913 Richardson was appointed Superintendent of the Meteorological and Magnetic Observatory at Eskdalemuir, an isolated place in Dumfriesshire intentionally far from any railway, 800 feet above sea-level with an average annual rainfall over 1400 millimetres. (The Observatory had been planned and built as a magnetic observatory under the National Physical Laboratory when electric traction rendered Kew unsuitable. As the National Physical Laboratory was, naturally, not used to running observatories and the Meteorological Office, naturally and actually was, the appropriate transfer to the latter was effected in 1910, with mutual goodwill and the approval of the Royal Society, which at that time controlled the National Physical Laboratory both scientifically and financially, and with the approval also of the Treasury which provided £1000 per annum to augment the income from the Gassiot Fund, for maintaining magnetic observations.) In 1914, after the outbreak of war, Richardson asked to be released, but the Annual Report of the Meteorological Committee for the year ended 31 March 1915, records 'after careful consideration the Committee felt themselves unable to grant facilities for which application was made by Lewis F. Richardson of the Observatory at Eskdalemuir, to join an Ambulance Corps'. He, himself, records, 'In August 1914 I was torn between an intense curiosity to see war at close quarters, an intense objection to killing people, both mixed with ideas of public duty, and doubt as to whether I could endure danger. After much difficulty I extricated myself from Eskdalemuir Observatory in May 1916 and joined the Friends' Ambulance Unit. In September 1916 I was attached to a motor ambulance convoy lent to the French army. We carried the wounded and sick of the 16-ieme division of infantry.' It was during this period, first in 1913-1916 in the bleak and humid solitude of Eskdalemuir and then in 1916-1919 in the solemn interludes of his ambulance work with the French army in France, conscious of the power of his new method of solving intractable equations that he developed the idea, 'which first took shape in 1911', of constructing 'a scheme of weather prediction which resembles the process by which the Nautical Almanac is produced, in so far as it is founded upon the differential equations and not upon the partial recurrence of phenomena in their ensemble'. It would have been quite easy for anyone to outline a scheme. Richardson's achievement was first to set out the dynamics and thermodynamics of the atmosphere—to make, in fact, the first text-book of dynamical meteorology—in the light of the, then recently acquired, knowledge of the upper air and of the roles of radiation and of eddy diffusion and to show how the resulting equations could be solved with a degree of accuracy at least as close as the accuracy with which the actual physical state of the atmosphere could be practically determined. The result was published in 1922 in his, now classical, book *Weather prediction by numerical process.*

He worked out, with meticulous care, the changes in six hours from 4h to 10h G.M.T. on 20 May 1910 deducible from an initial distribution for 7h G.M.T. on that day as given by V. Bjerknes in his *Dynamical meteorology and hydrography*. The method of computation involved the division of the atmosphere into horizontal layers of a thickness approximately 200 millibars and the layers into 'squares' whose sides were 200 km on the east and west and 3 degrees of longitude, just over 200 km in latitude 50°, on the north and south. The boundaries of the layers were the earth's surface and parallel surfaces at heights 2·0, 4·2, 7·2, 11·8 km. For convenience the squares were regarded as coloured alternately, like a chess-board, but red and white. The initial values of the components of momentum were tabulated for the centres of the white squares and the initial values of pressure, temperature and humidity at the centres of the red squares. The changes in the period of 6 hours, 4h to 10h, were then computed for each layer for two of the squares over central Germany, a white one for the momentum, and the red one immediately south of it for pressure, temperature and humidity—or water content of the layer.

The results of the computation were strikingly at variance with the observed facts. The most glaring error was in the computed change of pressure—a rise of 145 millibars in 6 hours, whereas in fact there was practically no change. This error was attributed mainly to a large spurious convergence of the initial winds. (In fact there was no possibility of obtaining observations of wind at different altitudes sufficiently accurate to eliminate this source of error until the development of radar methods after the second world war produced a dramatic increase of accuracy, which may now be good enough to make acurate computation practicable.)

Richardson realized that the scheme to be practical must satisfy the conditions, (*a*) that the computations could be completed quickly enough for the results to be ready and distributed well in advance of the time to which they referred, (*b*) that the additional cost of the observations and computing was less than the saving to mankind due to the information gained. Condition (*b*) includes implicitly the third condition (*c*) that the results must be more accurate than the predictions founded upon the combination of limited theory and the principle that atmospheric processes are broadly repetitive. As to (*a*), Richardson, assuming that an average computer might go perhaps ten times faster than he himself had done in France 'on a heap of hay in a cold wet rest billet', allowing for 1200 of the 3200 squares covering the globe to be squares where the weather is foreknown without computation and assuming that the observations from the 2000 squares were available in the computing room within a few minutes of the time at which they were made, estimated that it would need 64 000 computers to complete the calculation of the new distribution for the whole globe just before the time to which it referred.

As to (*b*) he estimated the value of the food production of the world as at least £1 000 000 000, 'so that a very tiny fractional saving would correspond to a large sum'. It is now many times greater and his comment is appropriate although the present cost of 2000 stations of which roughly half would be ship stations, would be of the order of £200 000 000 per annum.

As to (*c*) one of the compensating merits of Richardson's work was that it proved conclusively that the condition could not be fulfilled with the then existing means of observation and computation. It remains to be seen if the radical change due to radar and electronic computers will enable this condition to be satisfied.

After his return from France in 1919 Richardson worked again in the Meteorological Office, this time at Benson with W. H. Dines. There he devised a method of measuring the wind at different levels by its differential effect on steel spheres shot nearly vertically upwards so that when they returned to earth the point of impact could be noted. The steel balls varied in size from a pea to a cherry. At that time the only methods of measuring the wind in cloud were a very expensive and complicated one, originated by the French Military Meteorological Service, of sound-ranging on the successive explosions of charges carried by an ascending free balloon and the method, also expensive and applicable only to meteorologically low heights, of measurements made on a tethered kite-balloon; while for the wind above cloud there was the additional, but also expensive, method of flying an aeroplane at a measured air-speed through a line of smoke puffs produced by bursting shells fired at short intervals from a gun at a fixed elevation. All these methods had been used in the war, but apart from the matter of expense, safety considerations ruled out the first and third for normal peace-time use. Richardson's method involved complicated calculations but its cost per observation was of a much lower order of magnitude. It was, however, never brought into general use, mainly because with balls of a non-lethal size and muzzle velocities below the speed of sound (the latter condition necessary for the application of the theory) the height reached was not great enough to satisfy the synoptic meteorologist but partly also because Richardson left the Meteorological Office when it was transferred to the Air Ministry. He has recorded that he experienced 'a doubt so intense as to be distressing' in 1920 when he had to decide whether to leave the Meteorological Office. The doubt is not surprising, for during this period his mind had been very active both in devising inexpensive methods of measuring wind and temperature at different levels in the atmosphere and in the problem of atmospheric eddies or turbulence; and these activities seemed in no way to conflict with his 'intense objection to killing people'.

In 1919/1920 he published three important papers on turbulence. G. I. Taylor had used the letter K to represent 'the power possessed by the atmosphere in virtue of its turbulence of transmitting heat and momentum' and had given the names 'eddy conductivity' and 'eddy viscosity' to the quantities $\kappa = K\rho\sigma$ and $\mu = K\rho$ where ρ is density and σ specific heat. The value of K is not necessarily the same for heat as for momentum although Taylor had adduced evidence supporting the assumption of equality. Richardson introduced the name 'eddy diffusivity' for K and this has been generally adopted. He suggested, however, that a new quantity ξ for which he invented a new word 'turbulivity', was a better measure of turbulence than K. The turbulivity is defined by the equation $\frac{\partial}{\partial \rho} (\xi \frac{\partial \chi}{\partial \rho}) = \frac{\partial \chi}{\partial t} = K\frac{\partial^2 \chi}{\partial z^2}$ where χ is either potential temperature or mass of water or smoke per mass of atmosphere. He based his preference for ξ on the ground that it 'enables one to allow for the variations of density and turbulence in a simple and natural manner'. But 'turbulivity' has not been much used, and 'eddy conductivity' which he defined as $\xi/g^2\rho$, or $K\rho$ if there are no variations with height (analogous for the transfer of heat or smoke to 'eddy viscosity' for the transfer of velocity) is not often used. There are indeed in the literature of turbulence three meanings of 'eddy conductivity' viz. $K\rho\sigma$, $K\rho$ and K_h the last being the value of K for heat transfer.

Richardson showed that the values of K derived from observations of the spread of smoke and thistle- and dandelion-down across different types of land surface, of steamer smoke at sea and of the growth of cumulus cloud varied from 5 cm^2/s to 10^6 cm^2/s, much more widely than had previously been supposed, a fact which stimulated him later to apply a new method to the problem of turbulence. The outstanding result of his 1920 paper was his theoretical deduction of a criterion for the increase or decrease of turbulence in the atmosphere. The criterion depends only on the lapse-rate of temperature, the change of wind with height and the eddy conductivity, (c) and eddy viscosity μ_{xz}, μ_{yz}. The basis of the criterion may be put this way. Parcels of air, moving upwards and downwards in the atmosphere, cool or warm adiabatically, if the equalizing effect of radiation is negligible. If the lapse-rate in the atmosphere is less than the adiabatic lapse-rate the parcels of air can move up or down only at the expense of some of the energy of motion (which they already possess if they are eddying parcels). If the wind changes with height, this will, on the whole, give the eddying parcels additional spin and so add to their energy of motion. Turbulence will increase if the energy gained exceeds that lost. Richardson's criterion expresses in convenient form the ratio of loss to gain. The expression is now known as the 'Richardson number' and is denoted by *Ri*. It ranks as a fundamental dimensionless parameter in problems of the turbulent motion of the atmosphere. The full form of the expression is

$$Ri = cg\,(\partial T/\delta z - \partial T/\delta z_a) \Big/ \left[\mu_{xz} \left(\frac{\partial \bar{v}_x}{\partial z} \right)^2 + \mu_{yz} \left(\frac{\partial \bar{v}_y}{yz} \right)^2 \right]$$

where T is temperature, z is height, $\partial T/\partial z_a$ is the adiabatic value and $\bar{v}_x\ \bar{v}_y$ the mean components of horizontal velocity. The expression is usually simplified by assuming $c = \mu_{xz} = \mu_{yz}$ and is sometimes further, erroneously, simplified by substituting $(\partial \bar{V}/\delta z)^2$ for $(\partial \bar{v}_x\ \partial/z)^2 + (\partial \bar{v}_y\ /\partial z)^2$ where $\bar{V}^2 = \bar{v}^2{}_x + \bar{v}^2{}_y$

The condition for an increase of turbulence is $Ri < 1$.

Richardson realized that certain changes omitted as negligible (in comparison with the two principal changes), in particular the change due to molecular viscosity, might not always be negligible and it is now known that in certain conditions they are substantial. On the whole they tend to diminish the limiting value of the ratio, i.e. to replace 1 by $1 - q$ on the right of the inequality, q being a positive quantity. He also mentioned in 1952 that the use of $(\partial \bar{V}/\partial z)^2$ could result in a value for *Ri* double the correct value.

Richardson was led, through his observations, to stress the importance of the scattering of particles in the study of turbulence and in 1926 he contributed to the *Proceedings* a strikingly original paper, 'Atmospheric diffusion', treated from this aspect. He began, characteristically, by a gentle criticism of the application to the atmosphere of the eddy-diffusion equation of Fickian type, $\frac{\partial \chi}{\partial t} = \frac{\partial}{\partial x}\left(K \frac{\partial \chi}{\partial x}\right)$ on the ground that the 'constant', K, varied over very wide limits—a million times greater for the eddies of cyclonic size than for those in the wind. He pointed out that the rate of diffusion

—or separation of particles—increased with the distance between them. Briefly he says that if the position of an element of atmosphere is specified by reference to axes with a fixed origin, there is no limit to the mean value, taken over a large number of elements, of $(x - \bar{x})^2 + (y-\bar{y})^2 + (z-\bar{z})^2$. The quantity goes on increasing with time until cyclonic magnitudes are reached. But if the position of an element, 1, is specified by reference to another element, 2 (its neighbour), the mean of $(x_1 - x_2 - \overline{x_1 - x_2})^2$ plus similar terms in y and z reaches a definite limit independent of time, provided the time is neither too large nor too small. Consequently a more hopeful method of attacking the problem of diffusion by turbulence would be to use the distance between neighbours as the space variable and q, a function of the concentration as the dependent variable. If the concentration at a point x is $\nu(x, t)$ and at a point $x + l$ is $\nu_1(x, l, t)$ $q = \frac{1}{N}\int_{-\infty}^{+\infty} \nu\,(x, t)\,\nu_1\,(x, l, t)\,dx$ where N is the whole number of marked particles or elements. This quantity q has also been described as the probability of finding particles with separation l at time t.

Using l as the distance apart of neighbours, Richardson arrives at the following general equation of diffusion $\frac{\partial q}{\partial t} = \frac{\partial}{\partial l}\left(F(l)\,\frac{\partial q}{\partial l}\right)$

and shows that if $\frac{\partial \nu}{\partial t} = K\frac{\partial^2 \nu}{\partial x^2}$ then $\frac{\partial q}{\partial t} = 2K\frac{\partial^2 q}{\partial l^2}$

He then takes the values of K derived from observations of different scales of turbulence, estimates the corresponding values of l and finds $K = 0{\cdot}2\, l^{4/3}$. This was initially taken to warrant a value $0{\cdot}4\, l^{4/3}$ for $F(l)$ but the final conclusion was that $F(l)$ could be represented with reasonably close approximation by $0{\cdot}6\, l^{4/3}$ for values of l between 1 metre and 10 km. In his last paper in 1952 he stated that the corrected average value of the constant should have been 0·5 and not 0·6. As a result of these investigations, Richardson tried to find some modification of Fick's equation which would fit the facts but came to the conclusion 'that no differential equation in which position and time are the independent variables and mass of diffusing substance per unit length is the dependent variable, can describe horizontal atmospheric diffusion.' This seemed contrary to established results on vertical diffusion and in a subsequent note he resolved the dilemma by differentiating between the law of diffusion of a spreading cluster and the law applicable to the mean of many occasions taken at random or to the mean of wind components reckoned from the isobars as zero azimuth or the mean over a definite region for definite time. For the three latter a differential equation is appropriate: for the former neither the customary equation nor any generalization of it applies.

It is not a little surprising that the value of the exponent of $l^{4/3}$, deduced by Richardson from what might seem an inadequate collection of observations should be identical with that derived theoretically over 20 years later from reasoned hypotheses regarding the nature of the transfer of energy from large to smaller eddies in the atmosphere. He, himself, remarked relevant to this agreement 'the atmospheric observations could have been fitted passably by any index between 1·2 and 1·4. The 4/3 was chosen partly as a rough mean and partly because it simplified some integral.' The constant, 0·5 in the atmosphere, is sixteen times less on the sea, the latter determined from observations of articles floating on the sea-surface which give results fitting the same 4/3 law. The ratio of the constants is about the same as the ratio of the viscosity of air to that of water.

His last, 1952, paper on this subject disposed first of an objection to his deduction that the function ν is uniquely determined by q if the cluster is symmetrical, $\nu(x) = \nu(-\mathrm{x})$ and then to develop further the theory of the diffusion both of symmetrical and of skew clusters, and finally to specify two sets of experiments which if made on the surface of the sea, would determine two separate average values of the coefficient of $F(l)$. If the two values agree it would be evidence that the diffusion equation applies to skew as well as to symmetrical clusters.

The third main division of Richardson's work is described in a monograph *Generalized foreign politics*; it is, broadly, the application of mathematics to the relation between the various elements which determine 'what people would do if they did not stop to think'—to describe 'what would occur if instinct and tradition were allowed to act uncontrolled'. He begins by outlining the motives 'which lead a nation in time of peace to increase or decrease its preparations for war e.g. revenge or dissatisfaction with treaties, fear, rivalry, the tendency to economize expenditure' 'What the result of all these motives may be is not at all evident when they are stated in words. It is here that mathematics can give powerful aid.'

He then takes the case of two nations, one of which considers it must take steps to increase its defences because of the menace of the other. This can be formulated $\mathrm{d}x/\mathrm{d}t = ky$ where x represents defences and y menaces. And for the other nation $\mathrm{d}y/\mathrm{d}t = kx$.

If this were all, the system would be unstable since the solution of the equations includes a term Ae^{kt}. But if account is taken of the restraining effect of the cost of x and y and the opposite effect of 'grievances' the equations become $\mathrm{d}x/\mathrm{d}t = ky - \alpha x + g$, $\mathrm{d}y/\mathrm{d}t = kx - \beta y + h$
Unilateral disarmament corresponds to putting $y = 0$. The equations show that y will not remain zero and though x may decrease at first it will increase again later as y increases again.

He deduces, from the idea of a negative x suggested by 'the generalizing spirit of mathematics', that the opposite of war is not peace but an activity, 'co-operation', of which the most extensive form is foreign trade. Peace, 'in the sense of a mere tranquil inattention to the doings of foreigners resembles zero rather than a negative quantity'.

Deductions drawn from the two equations include the following. If the two nations have no grievances but, on the contrary, are both so satisfied that

g, h have negative values -2, -3, nevertheless if k, l each have the value $+2$ and α, β the value $+1$ the regime will be unstable. Conversely, if both nations have strong grievances, g, h being $+2$, $+3$ while k, l are each $+1$ and α, $\beta=2$ the regime will be stable.

He goes on to consider the representation of defence budgets and trade, and diagrams of comparative statistics for seven leading nations, from 1924 to 1938-9 and then the case of three nations and finally that of n nations. He illustrates his deductions with characteristic observations, often lightly illuminating the italicized general law, e.g. '*If each of the three pairs of nations be separately stable there remains the possibility that the triplet of nations may be unstable.*' Comment: 'those who observe dogs tell me that dogs which will play peaceably in all possible pairs, may yet quarrel when three are present together.' He refines the harsh conclusion, drawn from a consideration of the effect of improved communications between n equal nations that 'if κ_1 (the defence co-efficient between closest neighbours) remains the same while aviation increases, the formulae show that λ_1 (the greatest of the values of λ in terms in the value of x involving $e^{\lambda t}$) would increase; so that increase of aviation would tend to more instability or less stability or a change from stability to instability', by the comment, 'Is it not more likely that aerial contact with distant nations somewhat distracts attention from closest neighbours and thereby decreases κ'. He afterwards suggests for the mathematicians the unsolved 'problem of imperfect communications between n unequal nations'. He himself had arrived at the surprising conclusion that if the total of the defence coefficients of n nations was fixed, the regime would be least stable when the n nations were all equal.

Applying his results in 1938 to the statistics up to 1937 of the nine leading nations, he concludes that λ has the value $0{\cdot}4$ approximately, with the year as the unit of time and that the world was then unstable; but that after the war, or peaceful re-arrangement, to which instability would lead, 'on the average over the peaceful part of a long period such as a century during which aviation is increasing, the world may be expected to be just stable'.

A final chapter he devotes to questions from a critic and answers by the author in which the humour is not all in the questions nor the seriousness all in the answers. Two quotations are illustrative. *Critic:* A serious objection to your description is that it does not mention intelligent aggression planned by a leader as moves in a game of chess. *Author:* That is admitted; for a description of predictable tendencies cannot include anything so unpredictable as moves in a game of chess. Yet the acts of a leader are in part controlled by the great instinctive and traditional tendencies which are formulated in my description. It is somewhat as if the chessmen were connected by horizontal springs to heavy weights beyond the chessboard. . . *Critic:* So when grievances are negative and there are no threats, I suppose that everybody will do just as they please and the roast pigs will run about crying 'eat me'. *Author:* It pleases you to jest; but of course we both know that in a united world organization there will always be the need for compromise, discipline, law and police.

Richardson returned after his retirement to the study of this problem and revised and amplified the monograph to form a book *Arms and insecurity*. He also wrote and published many articles and papers with the same object of elucidating the effect of the different factors and with the hope of thereby contributing to the avoidance of war. The major work remained a microfilm. As Richardson said, 'It is still difficult, in 1953, to publish work on that subject. There are many anti-war societies, but they are concerned with propaganda, not research. There is a wide public interest in the subject provided it is expressed in bold rhetoric, but not if it is a quantitative scientific study involving statistics and mathematics. There is no appropriate learned society.'

The fourth main division was his educational work, in the early years for two brief periods, as a demonstrator in physics at Aberystwyth, 1905-6, and as demonstrator and lecturer in physics at Manchester, 1912-13; later in charge of the Physics Department of Westminster Training College, 1920-29, and then as Principal of Paisley Technical College and School of Art, 1929-40. At Aberystwyth, Sir David Brunt, then a student there, records 'he was keenly interested in designing a planimeter. He carried his model in his pocket, and would take it out and test and modify it in any free moment. He remained in my memory as a quiet friendly man, always ready to help in any difficulty with an experiment.' At Westminster and at Paisley, his ability as an experimenter, which he himself was disposed to underestimate, and his gift for the improvisation of apparatus were recognized by staff and students. The quality, mentioned at the end of the quotation above, was also emphasized in a note by one of his staff at Paisley, 'Often when we have been worried about some problem in practical physics, we have felt that if Dr Richardson had been available to us for consultation our troubles would soon have been over'. He was a clear lecturer but he had no enthusiasm for administrative work: it was 'a task to be performed with diligence and foresight, but to me was rather dreary'. Nevertheless, in a period of general civil retrenchment, he succeeded, by the full exercise of his diligence and foresight, in obtaining an extension of the laboratories at Paisley.

Richardson's scientific work was original, sometimes difficult to follow, sometimes illuminated by lucid unexpected illustrations. In turbulence and in the monograph on *Foreign politics* he was occasionally, and not unnaturally, groping, perhaps with a little of the confusion he had observed in his father. He was breaking new ground in both these subjects and had to find his way with the assistance of a knowledge of advanced mathematics gained as he went—not drawn from a stock obtained in his university career. In view of his inclination to explore new subjects—or even, as he says, 'bits of subjects'—his achievement might seem surprising if one did not realize his amazing and orderly industry. The Richardson number ranks in atmospheric turbulence with the Reynolds number. His development of the theory of the spread of pairs of particles lay dormant for twenty years until the law he had found was rediscovered; and is now finding applications in other than meteorological fields.

In preparing the following concluding paragraphs I have had Richardson's own notes, from which some quotations have already been made, and notes of some of his contemporaries, colleagues and friends kindly lent me by his wife. His notes, made at the age of 71, 'with the advantage of a psychological training' were intended to give 'a truthful account of motives as far as scientific research is concerned'. He had a strong curiosity. Doubt was mostly pleasant, but a distressing doubt in 1920 has already been mentioned; and 'for fifty years I have remained comfortably in doubt as to the existence of a future life, or as to which political party is the best.' He records an incident, which occurred when he was about four, and which may have influenced the development (or progress) 'of a sceptical and experimental attitude that later became habitual'. It is best described in his own words. 'My elder sister Catherine and her nurse came into the nursery wearing their hats.

L.F.R.: Where are you going?
They: We're going to the bank.
L.F.R.: What for?
They: To put money in.
L.F.R.: What for?
They: To make it grow larger.

I was left alone to think about this remarkable way of "making money grow larger". I took my money—a silver threepenny bit and a new farthing—found a trowel, dug a hole in the steep bank in the garden, put the money in and covered it over with earth. About three days later my sister and I together dug up the money. It was not any larger. I was grievously disappointed, and felt that the words of grown-ups were not always to be trusted.'

At the age of 55 he had his intelligence tested. It was 'high enough for observational science, but not for the higher flights of pure mathematics'. The tendency of his mental machine *almost* to run of its own accord made possible 'an intentionally guided dreaming. If the machine ran *entirely* of its own accord, one would have no control, and the dream would be out of touch with reality. . . It is the "almost" condition that is advantageous for creative thinking. . . In some ways it is a nuisance; for example I am a bad listener because I am distracted by thoughts; and I was a bad motor-driver because at times I saw my dream instead of the traffic.'

Research was the inevitable consequence of this mind-structure. Sometimes he was paid to do it—and liked being paid—but, paid or not, he went on with it; and used part of a legacy to facilitate it. 'I have often been anxious lest I was wasting the substance of my wife and children on my personal interests.' That accounts for the experiments and theories, but not for publication. 'Publication required another motive, namely the tendency to display. I have been aware from childhood that this tendency was strong in me, and that it was often unsuitable; so I have diligently suppressed it. Yet, when disciplined a tendency to display is surely useful.' I have quoted this, the only motive for publication given by Richardson in his notes; but I feel that his consciousness of the presence of this disliked motive made him overlook the fundamental compelling motives of sharing his discoveries and letting them contribute as greatly as possible to the well-being of mankind which all the evidence about his character indicates would be strongly operating motives with him.

He mentions that from childhood his hand trembled, which 'ruled out surgery as a career and hindered manipulations in physics'. . . He had a weak memory; at school and at Cambridge he disliked memory-subjects and even in mathematics which he used and enjoyed he could not 'memorize even the formula connecting $\cos^2\theta$ with $\cos 2\theta$.' He realized his solitary temperament. 'I hardly know what loneliness feels like: when solitary I am usually serene; when in a crowd, I am often embarrassed. . . After I had become a professional scientist and when I met obvious leaders, my usual tendency was to shy away from them. . . But by exception I attached myself to Karl Pearson and later to Sir Napier Shaw because my programme temporarily overlapped with theirs.' It was in fact the 'facilities, encouragement and praise from Sir Napier Shaw' which led him into meteorology, a subject which 'had developed by 1926 into something like a passion. In order to attend to rival ideas I had to make a deliberate break with meteorology.'

'Gregarious readers will probably think such a standoffish temperament is a deplorable fault and an unmitigated disadvantage. It certainly does prevent its possessor from collecting disciples and founding a "school". Let me, however, point out that the same solitary temperament enables him to persist in unpopular researches such as those on the cause of wars' . . . and then referring to his break with meteorology. 'By me some such change had long been dimly foreseen. In the midst of my Cambridge course on natural science I was hesitating whether to specialize on the physical or on the biological when someone, probably my friend W. E. Agar (afterwards F.R.S.) told me that Helmholtz had been a medical doctor before he became a physicist. It thereupon occurred to me that Helmholtz had eaten the meal of life in the wrong order, and that I would like to spend the first half of my life under the strict discipline of physics, and afterwards to apply that training to researches on living things. I kept this programme a secret. The distant aim broke out temporarily in 1906 when I sold my physics-books in order to raise money to go and see Professor Karl Pearson and learn about statistical proof.'

Naturally the Society of Friends was a persistent influence, 'with its solemn emphasis on public and private duty . . . its condemnation of war pulled me away from the many warlike applications of physics.' His earliest paper relevant to the avoidance of war was written when he was with the motor ambulance convoy in France. 'There was no learned society to which I dared to offer so unconventional a work (*Mathematical psychology of war*). Therefore I had 300 copies made by multigraph, at a cost of about £35, and gave them nearly all away. It was little noticed. Some of my friends thought it funny. But for me it was quite serious, and was the beginning of the investigations on the causes of wars which now occupy me in my retirement.'

The tendency of his 'mental machine' probably explains why he sometimes appeared abrupt in manner, otherwise inexplicable in one whose quality is indicated by such records of impressions, over the years, on those who knew him as the 'very kind heart' at King's, the 'readiness to help' at Aberystwyth, the 'so very lovable and always so kind' at Westminster, 'his kindness and sincerity' at Paisley, and the 'beloved by all' in the motor ambulance convoy in France, where 'perhaps his greatest contribution was his demonstration of the dignity of service. His simplicity and integrity transformed into acts of permanent worth the performance of the most menial of the tasks which fell to our lot in those dreadful days.'

My own recollections of Richardson began with incidents during a visit to Eskdalemuir in the winter forty years ago. When official accommodation was provided there, in a wing of the Superintendent's House, for research or official visitors a condition was included in the formal arrangements that meals for visitors should normally be served in the visitors' sitting room. We compromised on that. I can see him, showing me one evening, to my admiration, his arrangement for keeping his scientific papers, documents and manuscripts in such order that he could lay his hands at once on any one he wanted to refer to. Another evening he disappeared—for a long run in the darkness and silence of the night, to keep his body and his mind also in good order. In the 1920's I met him often; what remains prominent in my memory is the unselfishness of Richardson and his wife in taking into their home the children of a missionary serving at that time overseas. Indeed I think the keynote of his life is given in a sentence from a tribute by one who was a fellow undergraduate 'he thought the value of life consisted in the amount of self-denial you paid for it'.

E. Gold

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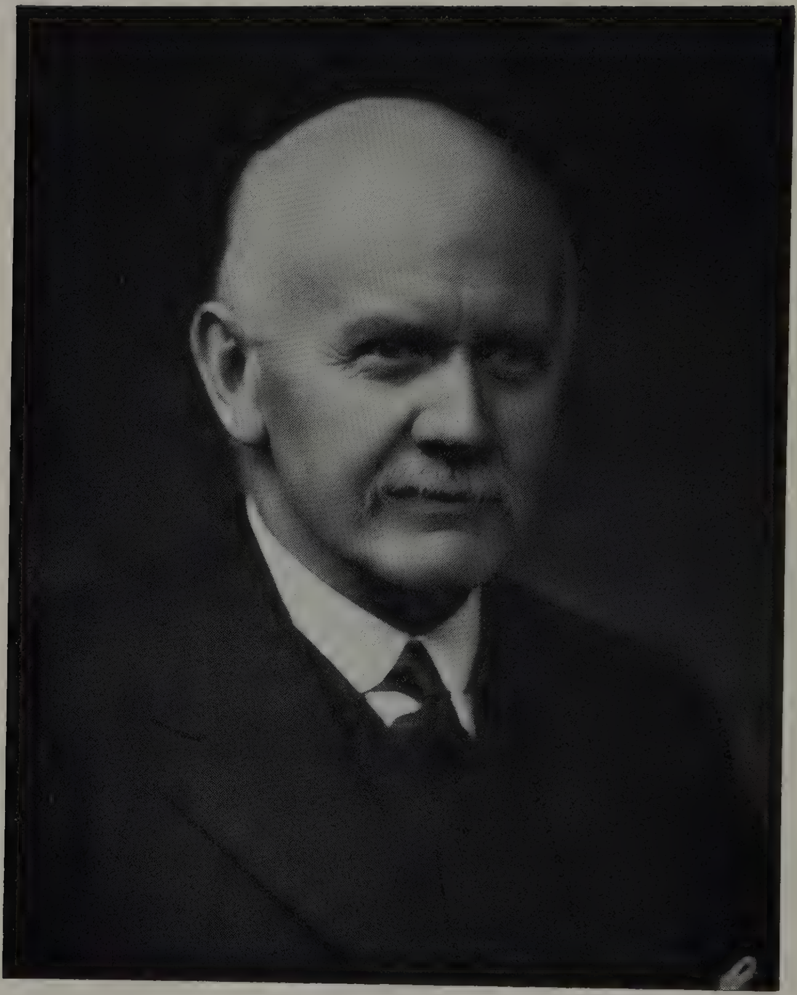

N. V. Sidgwick

NEVIL VINCENT SIDGWICK

1873-1952

Nevil Vincent Sidgwick, who died on 15 March 1952, came from a highly gifted family. His ancestors were Yorkshire farmers, but his great-grandfather became a successful cotton spinner in Skipton. His grandfather, William Sidgwick, broke away from the family business and was sent to Cambridge, where he became a Wrangler. He was Headmaster of Skipton School until his death in 1841. He had three sons and one daughter. The eldest son, William Carr Sidgwick, Nevil's father, was born in 1834. The two younger sons, Henry and Arthur, were born in 1838 and 1840. Henry Sidgwick became the famous Professor of Moral Philosophy at Cambridge. Arthur Sidgwick was elected to a fellowship at Trinity College, Cambridge, in 1864, but soon afterwards was appointed an assistant master at Rugby School, where he remained for fifteen years. He was elected to a tutorial fellowship at Corpus Christi College, Oxford, in 1882, and later became University Reader in Greek. Mary Sidgwick, their sister, married in 1859 Edward White Benson, who was then an assistant master at Rugby, and later became Archbishop of Canterbury.

William Sidgwick's widow went to live near Bristol after her husband's death, but moved to Rugby in 1853 and sent her two younger sons to Rugby School. William Carr Sidgwick, who had been at school at Blackheath, went to Oxford as a scholar of Corpus Christi College, took a First Class in Greats in 1856, and was elected a Fellow of Merton College in 1857. He was ordained in 1860, but like his brother Henry and other contemporaries at Oxford and Cambridge became increasingly conscious of the fact that he could not subscribe to the religious obligations which were then demanded of Fellows. He finally took advantage of the Clerical Disabilities Act (1870), and relinquished Holy Orders in 1871. Before then he had decided to marry, in spite of the ordinance providing that 'every Fellow who shall marry shall thereupon vacate his fellowship'. There is a note in the College records, dated 6 February 1872, 'that Mr Sidgwick be allowed a year of grace in contemplation of marriage'. He married in March 1872, and Nevil Sidgwick was born in Oxford on 8 May 1873 at Crescent Villa, Park Town (now 8 Park Town), which was then on the northern outskirts of Oxford.

After his marriage William Carr Sidgwick was for a short time a schoolmaster in Guernsey, but he returned to Oxford and continued to teach and lecture for some years. He became a member of Oriel College and Lecturer in Logic and Political Economy. In 1881 he applied for the Professorship of

Moral Philosophy and Political Economy at Liverpool. The testimonials that he submitted might have been written of his son. Mark Pattison, the Rector of Lincoln College, referred to him as 'a man of acute intellect, shrewd practical judgment and unusual powers of expression and exposition'. Magrath, the Provost of Queens College, wrote that 'for lucidity of exposition he has no superior if he has an equal in the University and his kindness to and interest in his pupils has endeared him to several generations of students'. All his testimonials dwelt on these qualities; but he was not elected, and left Oxford for London where he was called to the Bar, and practised journalism for a few years before finally retiring, a disappointed man, to Rugby, where he lived from 1888 until his death in 1910. It is not easy to account for his lack of success, and Nevil Sidgwick himself was extremely reticent about his father even to his closest friends. But there is a hint in George Sainsbury's *A scrap book*: 'My tutor,' he wrote, 'the late William Sidgwick, cleverest perhaps, if not exactly wisest of his singularly brilliant family' . . .

One is left in no doubt from whom Nevil Sidgwick derived his powers of thought and speech. 'My dear Nevil, there are times when you remind me exactly of your father' said an uncle who was getting much the worst of an argument. But he owed to his mother his love of science and many of the best points in his character. They were devoted to each other: he was never out of her thoughts, nor she out of his. Her death in 1915 was the severest blow of his life.

She was Sarah Isabella Thompson, daughter of John Vincent Thompson, Serjeant at Law, whose brother was General Thomas Perronet Thompson, F.R.S. Her sister, Philothea, married Sir Benjamin Brodie, F.R.S., Professor of Chemistry at Oxford from 1855 to 1872. They were descended on their mother's side from a Swiss family called Perronet. Towards the end of the 17th century one branch of the Perronets settled in France and another in England. Jean Rodolphe Perronet (1708-1794) was a celebrated French engineer and a Foreign Member of the Royal Society. His English contemporary Vincent Perronet (1693-1785) was Vicar of Shoreham and an intimate friend of John Wesley.

Nevil Sidgwick was sent to Summer Fields School, near Oxford, for one year from 1885 to 1886, when his parents were living in London. Previously to that he had been educated entirely at home, mainly by his mother, who early imbued him with that love and wide knowledge of botany and natural history which was one of his most marked characteristics and which was to add so much to the interest of his travels. He entered Mr Scott's House (School Field) at Rugby School in September 1886, but became a day boy in 1888 when his parents moved to Rugby. He must have been an ungainly boy, physically unattractive and useless at any kind of game.* This does not seem to have affected him at all, even in those days when most small boys at public schools had to contend with serious difficulties if their main assets were their brains rather than their athletic accomplishments. Possibly his wit was too biting and his good humour too disarming for his tormentors, if indeed there were any. There is no indication that he had any difficulty in making friends. He looked back to his school days with pleasure, and was an enthusiastic member of the Governing Body from 1933 to 1944. A letter written by Mr Scott to his mother during his first term may give some indication of him:

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* In later years he became an expert skater, and a competent player of golf, to which he was introduced by one of his Magdalen pupils.

> 'Nevil has made his debut. He has plenty of wit and also of inattention. . . Plenty of blunders, all of a healthy nature. . . What strikes me most is what must strike anybody; his ease and general grasp of all he does.'

Rugby School was then in the forefront of the new movement for the teaching of science at public schools, but there was no concentration on science to the exclusion of literary subjects. Throughout his school career Sidgwick studied classics as well as science. He rapidly rose in the school, and in the winter term of 1891 was placed third in the Classical Sixth form, and third in the Science Sixth. He sat for a classical scholarship at Oxford in 1891, but failed. He then resolved, with his mother's encouragement and his father's acquiescence, to devote himself to science. In 1892 he was elected to an open scholarship in Natural Science at Christ Church, Oxford. Two of his contemporaries were elected at the same time to science scholarships at Balliol and Trinity. Sidgwick applied to the headmaster for the customary half holiday in recognition of these successes, but met with a refusal. The custom was confined to the winners of classical scholarships!

Sidgwick entered into residence at Christ Church in the autumn of 1892. He was fortunate in his tutor, Vernon Harcourt, the Dr Lees Reader in Chemistry, who was a pioneer in the new domain of physical chemistry, and one of the first to measure the velocity of chemical reactions and to study the conditions that determine it. The brunt of the teaching of chemistry was borne at that time by the Colleges, and Sidgwick did the whole of his practical work in the Christ Church laboratory. He was placed in the First Class of the Honour School of Natural Sciences in 1895. He then decided to read 'Greats', and in 1897 obtained another clear First Class in Finals. One of his examiners, Dr Rashdall, wrote in 1902, when Sidgwick was a candidate for the vacant Dr Lees Readership:

> 'I retain a very distinct and a most favourable impression of his work. That work was done after he had for a long period given up Classics and devoted himself to Natural Science. To take a not doubtful first class in such circumstances was a remarkable feat. It was accomplished chiefly by his work in the philosophical subject. His point of view was still perhaps, as was natural, rather that of Natural Science than of Metaphysics, but he showed abundant evidence of high philosophical capacity and contrived to make it quite plain that he was one of the

> two or three ablest men—not impossibly the ablest—of his year. His *viva voce*, in which he maintained the positions he had taken up in the papers with great tenacity and dialectical skill under a severe cross-examination by an accomplished Metaphysician, I look back upon as one of the most brilliant performances of the kind that I have ever had the pleasure of witnessing.'

After this truly remarkable feat, which is never likely to be repeated, Sidgwick demonstrated in the Christ Church Laboratory for a year, and was then elected to a Dixon scholarship which enabled him to go to Germany. He went first to Ostwald's laboratory in Leipzig, in order to acquire modern techniques for the physical study of organic compounds. Unfortunately he fell ill there, and returned to Oxford, where he spent the summer of 1899 investigating the action of alcohol and other organic substances on stannic chloride. He then went to Germany again in the autumn to work under von Pechmann in Tübingen, where he spent two happy and fruitful years, becoming a fluent German scholar, and carrying out a research on derivatives of acetone-dicarboxylic acid for which he obtained the degree of D.Sc. in July 1901. This was published as a dissertation (dedicated to his mother) in Tübingen, but only appeared in the *Berichte* in 1904, the delay being caused by von Pechmann's illness and death.

Before he left Tübingen Sidgwick was elected to a Fellowship at Lincoln College, where he went into residence in October 1901 and remained for the rest of his life. Apart from his unsuccessful candidature for the Dr Lees Readership in the following year, and a not very serious application for the Professorship of Inorganic Chemistry in 1920, when Soddy was elected, he appears never to have sought any other appointment, or seriously considered any invitation. Lincoln College became his cherished home, and Oxford the centre of his life's work.

I well remember my first meeting with Sidgwick in 1905. He was then responsible for teaching the chemists at Magdalen as well as at Lincoln. I had been reading mathematics for a year, and was told at the end of the summer term to go and introduce myself to the man who would be my tutor for my remaining years at Oxford. I called to see him in the rooms which he was to occupy for nearly 50 years. An elderly looking man, almost bald, with a fringe of grey hair, rose and greeted me pleasantly. It was not till long afterwards that I realized he was then in his early thirties. He asked when I had last done any serious chemistry. I told him. 'Oh', he said, 'in that case you must settle down to some serious reading in the long vacation.' Observing, perhaps, a lack of enthusiasm in my face, he turned to his shelves and picked out a new-looking book entitled *Chemical statics and dynamics* by Mellor. 'This is an important subject' he said, 'but the book is rather too mathematical for me. I wish you'd take it away and see how many mistakes you can find in it.' I believe that this was the first time that any senior man had indicated that I might know more than he did about something that was worth knowing, and the first time that anyone had suggested to me that there might be mistakes in a printed book of science. I took it away: an early discovery of a mistake in the first chapter aroused a detective spirit and led me to consult works of reference; and I returned in October with a list the length of which surprised my tutor as well as the author (who wrote a charming letter of acknowledgment), and I learnt a lot of chemistry in the process. This was characteristic of Sidgwick's method of dealing with his pupils. He was a great tutor, except of a few men whom he did not understand and whose difficulties did not appeal to him. Most of his pupils will always remember him with gratitude and affection, although they may find it hard, as I do, to describe the unusual nature of his influence. He did not appear to take very much trouble. He read one's essays casually and uncritically, but with stray illuminating comments. He would spend the rest of the time discussing irrelevant topics, often unconnected with chemistry. He would advise one to go to lectures to see what they were like, but did not blame one for irregular attendance. I did not even regularly attend his own lectures on the organic chemistry of nitrogen; I found it more convenient to borrow the typescript from which he read them, and study it at leisure. He was never caustic about one's ignorance, so long as he detected a desire to improve. He was nearer than anyone else I have known to Stephen Leacock's description of an Oxford tutor in *My Discovery of England*:

> 'I understand that the key to this mystery (of an Oxford education) is found in the operations of the person called the tutor. It is from him, or rather with him, that the students learn all that they know: one and all are agreed on that. Yet it is a little odd to know just how he does it. "We go over to his rooms", said one student, "and he just lights a pipe and talks to us." "We sit round him", said another, "and he simply smokes and goes over our exercises with us." From this and other evidence I gather that what an Oxford tutor does is to get a little group of students together and smoke at them. Men who have been systematically smoked at for four years turn into ripe scholars.'

Sidgwick did smoke in those days; a pipe was hardly ever out of his mouth in waking hours except for meals; and by the time he had smoked at one for three years one had a pretty firm foundation of a liberal scientific education on which to build if one had a mind to. But he fell short in one respect: he was not inspiring as an investigator in the years of which I write. His researches, such as they were, were haphazard. At first he continued, in a desultory fashion, his work on the organic compounds of tin, but soon abandoned it without publishing any results. He lost interest in the subject so much as actively to discourage one of his pupils from pursuing it on the reasonable ground that organo-metallic compounds could be poisonous. But in 1923, when their importance to the study of valency became obvious to him, this objection seemed to lose its force and all his pupils were set to work on them, fortunately without mishap. The fact is that Sidgwick, though an accurate, was never an ardent experimenter. He liked to have a band of disciples round him to do most of the work in the laboratory. He was not even very good in selecting topics or advising on methods of research; where he excelled was in the interpretation of results; he could see further than could most of those who produced them. But he had no clear line of work before him until much later. His scientific reputation in 1914 was largely confined to Oxford, and even there mainly to the younger men; some of the older were inclined to regard him as an organic chemist gone wrong. To the outside world of the Chemical Society he was known rather as an acute critic and the author of the *Organic chemistry of nitrogen* than for the merits of his original work. The coveted distinction of the Fellowship of the Royal Society, for which he yearned, seemed terribly slow in coming; and his closest friends felt that there was some danger of his becoming a disappointed and disheartened man, like his father.

The *Organic chemistry of nitrogen* was published in 1910. In the preface Sidgwick wrote: 'It is becoming generally recognized that organic chemistry cannot be treated satisfactorily without reference to those questions of physical chemistry which it involves. To attempt a separation of the two is to refuse all the assistance which can be derived from what is the quantitative side of chemistry.'

The book was not enthusiastically received by the scientific press; one reviewer went so far as to criticize the author's English—a bold man indeed. It was, however, a great success. The clear, logical and convincing discussions of controversial matters were of a kind that had never been seen before, and gave the book an altogether outstanding quality. Its educational value was immense. Many of the theories put forward did not survive the passage of time; but the masterly assembly of facts remained invaluable.

The edition of 1250 copies was soon sold out. Twenty-five years after publication the book cost second-hand four times as much as new; and I have been told that when the copy in the library of the Kodak Company was lost it cost thirty-three dollars to replace it. Plans for a second edition were made by Sidgwick in 1922; but the revision went slowly. He soon became absorbed in his work on valency, and ten years later he gladly handed over the responsibility for an extensive revision of the work to T. W. J. Taylor and Wilson Baker. The second edition was published in 1937 and has been several times reprinted.

The year 1914 was a turning point in Sidgwick's career. He was one of the members of the party chosen to represent the British Association for the meeting held in Australia. This was the first of the many travels overseas which added so much to his enjoyment of life. It was an especial stroke of good fortune for him that Rutherford was a fellow passenger on board the ship. He fell at once under the spell of Rutherford's genius and gaiety. He conceived for him a respect which ripened into a deep devotion in later years. He could not bear at any time to hear even a playful criticism of him; and if anyone was so rash as to differ from Rutherford in print, one felt that

Sidgwick would gladly have had the offending article or book burnt by the common hangman! Any pronouncement of Rutherford was right in Sidgwick's eyes; he brought, for instance, the whole weight of his classical erudition in support of Rutherford's suggestion of diplogen as the right name for heavy hydrogen. He felt that this was too serious a matter to be left to the discoverer. However, he took the final decision philosophically: we can at least agree, he said, to denote the new hydrogen by the symbol D.

Perhaps it was Rutherford who supplied the spark that released into its right channels the pent-up energy of Sidgwick's mind. Certainly the curve of his achievements rose thenceforward like the pressure of an explosion. But the period of induction was lengthened by the war of 1914 to 1918, during which Sidgwick, who was clearly unfitted though not unfit for any normal form of military service, stayed quietly at Oxford, acting as an unpaid consultant to the Department of Explosive Supplies. The possibility of manufacturing acetone from alcohol by a three stage catalytic process, alcohol-aldehyde-acetic acid-acetone, had been suggested by W. H. Perkin. It was investigated first by B. Lambert and then by Sidgwick, who took over entire responsibility for the work when Lambert left to join the Army. Eventually a large-scale plant was built at Warrington, but formidable difficulties were encountered, and on one occasion a serious explosion caused the death of several men. The final stage of the conversion of acetic acid into acetone was never satisfactory, but the process was used throughout the war for the manufacture of acetic acid. I know of no other clear practical outcome of Sidgwick's war work; but his advice was often sought and always valued, and he himself gained from the experience and from the widening of his friendships.

At the end of the war Sidgwick returned to the study of the solubilities and vapour pressures of isomeric disubstitution products of benzene. In a paper published in 1915 it had been shown that whereas the solubilities in water of the isomeric cresols and toluic acids are very similar, the nitrophenols are abnormal in that at a temperature at which the para and meta nitrophenols are completely miscible with water in all proportions, not more than 1% of the ortho compound will dissolve, which was all the more surprising since the ortho compound has by far the greater vapour pressure. The conclusion that ortho-nitrophenol has a different molecular structure from the para and meta compound seemed inescapable.

In a series of interesting contributions to the *Journal of the Chemical Society* from 1922 to 1924 Sidgwick and his collaborators recorded the results of similar investigations into the solubility and vapour pressure of other derivatives of benzene. The general conclusions were that the abnormality of ortho compounds is always associated with the presence of OH, NH_2 (or substituted NH_2), or CO_2H; that the degree of abnormality depends on the nature of the second substituent, being greatest, for example when it is CHO, quite distinct when it is Cl, and non-existent when it is CH_3; and that the probability was that hydrogen in the active substituent forms a co-ordinate link with some atom of the second substituent. All this work was a prelude to that which was to come.

In 1922 Sidgwick was elected a Fellow of the Royal Society. The distinction did not, in his case, mark the end of his best work for science; it marked the beginning. His horizon extended to embrace the whole of chemistry. He was then 49 years of age, full of vigour, and quite unchanged in appearance from what he was twenty years previously. But his friends observed a change in other respects: he became more mellow, more confident, more masterly, and yet more tolerant.

Ever since his first discussions with Rutherford on the way to Australia in 1914 Sidgwick had been interested in the bearing of the nuclear theory of the atom on problems of chemical combination, but he had taken no direct part in the various speculative theories that were put forward after the discovery of the atomic numbers of the elements. The position in 1921 was that there was general acceptance of the view that in the simpler ionic compounds there is a transfer of an electron, or electrons, from the positive to the negative atom, so that each ion is surrounded by a number of electrons equal to the atomic number of the nearest inert gas. This theory, which was first given a precise form by Kossel in 1916, gave a fresh meaning to the century-old idea of electro-valency; but it did not apply to non-polar compounds. G. N. Lewis, on the other hand, had made an advance of fundamental importance in the same year by suggesting the possibility that in non-polar compounds atoms could share electrons in such a way as to bring about a more stable grouping of the system as a whole. The most stable grouping, in his view, was one of eight valency electrons arranged in cubic formation. This 'octet' theory, which was elaborated by Langmuir and others, received considerable attention; but it was abundantly clear by 1921 that the postulate of stationary electrons was not in harmony with physical evidence. Neither theory, nor any combination of them, could account at that time for the formation and stability of the complex inorganic compounds which Werner had been studying for many years, but in which chemists had taken singularly little interest until he succeeded in predicting and demonstrating that certain of them could be resolved into optically active components.

This was obviously an unsatisfactory state of affairs, which, in Sidgwick's view, would remain unsatisfactory until more was known about the distribution and function of electrons in atoms. The further information for which chemists were waiting was provided by Bohr's note on 'Atomic structure' which appeared in *Nature* in March 1921, and by the publication of his extended *Theory of spectra and atomic constitution* in 1922. The first-fruits of Sidgwick's heightened interest in the subject appeared in his note on 'The structure of basic beryllium acetate' and in his paper on 'Co-ordination compounds and the Bohr atom', which were published in 1923. In June of the same year Lewis visited Oxford and stayed with Sidgwick. They both attended the meeting of the British Association at Liverpool, where Sidgwick opened a discussion on 'The Bohr atom and the periodic law'. Their interchange of views, and the publication of Lewis's *Valence and the structure of atoms and molecules* later in 1923 provided Sidgwick with an additional stimulus. He saw his way at last to develop an electronic theory of valency which could be applied to all the known facts without infringing the conditions which physical research showed to be necessary. But to do so it would be necessary to survey the whole field of chemistry.

To this formidable task he now addressed himself. It was a task for which he was pre-eminently fitted. It needed a man with an acute penetrating intellect, an orderly and critical mind, a prodigious memory, an exceptional power of assimilation and a dogged perseverance. All these qualities Sidgwick possessed; and he buttressed them by a determination to synthesize the sciences of physics and chemistry; never to stretch his inductive logic so far as to reach conclusions which, however attractive to the chemist, would be regarded as untenable by the physicist. He constantly inculcated this practice in his teachings and writings; thus in his introductory public lecture on 'The relation of physics to chemistry' delivered at Cornell University in 1931 he said:

> 'The ultimate object of the chemist is to express his conclusions in physical terms, but he must remember, if he tries to do this, that these terms have already a very elaborate and precise connotation; every concept which he uses involves a series of definitely established properties. That in fact is why it is so important to be able to use them. But it is essential to use them rightly. The chemist must not employ the language of physics unless he is willing to accept its laws. Within these laws a certain latitude of interpretation is left to him, and some tentative physical suggestions may be put forward unsupported by physical evidence, provided the physical evidence does not contradict them. On this last point no exceptions are allowed. The chemist must resist the temptation to make his own physics; if he does, it will be bad physics—just as the physicist has sometimes been tempted to make his own chemistry, and then it was bad chemistry.'

Sidgwick's *Electronic theory of valency* was published in 1927. In was rightly said of it when he was awarded a Royal Medal in 1937 that for the first time the most diverse structural phenomena covering the whole field of chemistry were rationally systematized, and that it had more widespread influence on the views of chemists in this country than any other of this generation.

The most original part of this now classic work, of which some 10000 copies have been sold, was that which dealt with co-ordination compounds. His original paper on 'Co-ordination compounds and the Bohr atom' was indeed a carefully laid foundation stone, now almost lost in the magnitude of the structure that has risen around it. He expounded the results of five years of further study and research in a brilliant Presidential Address to Section B of the British Association at the meeting at Leeds in 1927. Nothing is more illustrative of his powers of induction than his conclusion that the co-valency of carbon atoms did not differ fundamentally from the co-ordination of inorganic compounds. He reached this conclusion after an exhaustive study of all the relevant facts that he could find. It provided a firm basis for the general electronic theory of valency. It followed that the special theory of co-ordination which Werner had evolved with so much skill and insight, was unnecessary; and it became clear that the co-ordinate link is of the first importance throughout the whole of chemistry. His views have since been modified in detail; but their general validity remains unimpaired.

In the last chapter of his book Sidgwick considered in a preliminary way the application of the new principles to the individual elements; and he indicated in the preface his intention to develop this in a second volume. Over twenty years were to elapse before he achieved this ambition.

This highly productive period of his life from 1920 to 1930, during which Sidgwick published the book which brought him international fame, and was also responsible wholly or in part for over forty original contributions to scientific journals, was not without major relaxations. In 1924 he visited Canada with the British Association, and after the final meetings in Toronto crossed the prairies and the Rockies to the West Coast before returning to Oxford. In 1929 he had an even more extended tour to South Africa with the British Association, ending by travelling north to Livingstone by train, then by road to Dar-es-Salaam, and then after two days in Zanzibar sailing home via Aden and Suez.

> '1 Oct. 1929
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> I have had an excellent time and everything went well—the supposed hardships of a 1200 mile drive resolved themselves into one night in a very good tent. The wild beasts that were going to devour us were reduced to one lion which came by our bedroom in Abercorn, but so quietly that it did not wake us up. The country was extraordinarily interesting and the views in Tanganyika magnificent, and it was quite as interesting to meet and hear the views of all sorts of people—Government officials, farmers, lorry drivers—whom we met or stayed with en route. . . I have also had a drive round Aden by moonlight, and been presented to the Sultan of Zanzibar, and seen seven giraffe in the bush, and generally enlarged my mental horizon. . .'

He was soon to have his mental horizon still further enlarged in the most agreeable manner. Towards the end of 1930 he was invited to spend a semester at Cornell University as George Fisher Baker Non-Resident Lecturer in Chemistry. He accepted with alacrity, sailed to New York in January 1931, spent a fortnight there, visited Princeton and settled down in Ithaca early in February, 'with every luxury that an American laboratory can supply. Two offices, four telephones, a private laboratory, and a stenographer, all to myself. . . It is a wonderful place, with a great deal of good work going on, and everybody is most kind, so that I can see that I am going to have a very pleasant time here.' He made an immediate impression on old and young alike, and found to his joy that he was as acceptable to American students as he was to Oxford undergraduates. 'We found him,' wrote one of them after his death, 'to be cosmopolitan in the very best sense of the word . . . a man of wide experience not only in chemistry and related sciences, but also in the humanities; widely travelled, with acquaintances and friends in all parts of the world. . . More than one of us found in later years that Professor Sidgwick was one of the most important influences under which we came in Cornell.' Very soon the custom grew up of having tea with him in his laboratory, with some six to twenty in attendance, faculty members and students alike; and later on he received the rare compliment of being invited to live with a group of students in their fraternity (Telluride) house for the last two months of his stay. 'So I now find myself in a very comfortable home and excellent company . . . I certainly never expected that I should, as it were, go back to College again as an undergraduate, nor, to be perfectly frank, that if I did so at my age I should enjoy it as I do. I have just succeeded in learning all their 27 names, and I am no doubt rapidly acquiring the manners and language of the country.'

During his time at Cornell Sidgwick was much in request for lectures elsewhere, so 'I propose to see some of the country gratis in this way'. He gave more than he expected, and got a little nervous of repeating himself. 'I have quite a good paper on carbon monoxide and divalent carbon' he wrote in March 1931, ' and I am wondering how often it can be used before it wears out.' He need have had no fear. Each fresh audience listened to him with interest as he skilfully expounded the new theories, and in each fresh place he enlarged his friendships.

When his time came to an end he set out on a 10000 mile tour to the West, which included a 'hectic little lecture tour of four days', five days at the Grand Canyon, where he spent the night at the bottom, coming up the next day 'thoroughly desiccated and decorticated (I have said this to someone else, but I think it is good enough to use again . . .)', and a week at Pasadena, before turning east again at San Francisco and returning via Yellowstone Park, Buffalo, Ottawa and Quebec. He was the perfect tourist, indefatigable, interested in everything and everybody, and always ready at the end of a long day to send his friends vivid descriptions of his experiences.

> 'Aug. 14, 1931 Old Faithful Hotel, Yellowstone Park
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> I have now come to the last stage of my American tour. . . It has really been a wonderful time, and there are not many of the better known sights of the West that I haven't seen—as well as some less famous ones. . . But none of the places I have seen or am likely to see is as strange as this. It is an enormous hotel built of logs—said to be the largest log building in the world—anyhow it is 180 yards along the passage from my room to the porch in the centre, so it is > 360 yards long. This stands in a high (7300 ft. wide) but in many ways rather ordinary Rocky Mt valley, surrounded by pine clad hills. In the valley

> are several white sandy-looking areas, on which pools and small grey hummocks are scattered. From each of them a column of steam goes up, and at intervals, when there is an eruption, rises hundreds of feet into the air. I counted 39 distinct columns at one time from the hotel porch ,yesterday. Old Faithful, one of the biggest, is only 200 yards away from the hotel and goes off every 64·5′—its next time is marked on a clock face in the porch. I have seen it erupt 10 times since I came here yesterday, and it is extraordinarily beautiful, changing each time with the light and the wind . . . I have seen Salt Lake City, which is in very fine country, with Mts rising 7000 ft above it within 10 miles. I got there in a heat wave, went away up to 8500 ft in the mountains to a cabin among beaver dams and rainbow trout, and came back to the City with the break-up of the heat wave through a regular tropical thunderstorm. Neither nature nor man seems to do anything by halves in this country. . .'

He got back to Oxford in September 1931, hoping to be free from Committee work, and to concentrate on his researches and books. Earlier he had written:

> 'When I do return I shall be off all Committees and shall be able to refuse to go back on them on the ground of the 4(!) books and new Edns. which I hope to get out in the next few years. I have come to the conclusion that while many others are as good or better than I am at committees, I can do this molecular structure business better than most and had better stick to it. Of course lecturing out here is bad for one's modesty.'

He was wrong in one respect: few men were better Committee men than he. The Delegacy of the Clarendon Press could not do without him; nor could the Bodleian Library. London called him too: he had already promised to serve on the Advisory Council of the Department of Scientific and Industrial Research, and was Chairman of the Chemistry Research Board from 1932 to 1935. He became a member of the Council of the Royal Society from 1931 to 1933, President of the Faraday Society from 1932 to 1934, and President of the Chemical Society from 1935 to 1937. Before then he had been made Chairman of its Publications Committee, because, so someone said, they could not find anyone else who could make sensible comments on all the papers submitted.

Other honours came his way in 1935. He was created C.B.E., received the honorary degree of Doctor of Laws from Liverpool University, and was elected by Oxford University to a supernumerary Professorship of Chemistry, 'without pay or duties; but it is a very pleasing sign of recognition'.

His first Presidential Address to the Chemical Society in 1936 opened with the following highly characteristic paragraphs:

> 'There are two ideas about the progress of science which are widely prevalent, and which appear to me to be both false and pernicious.

> The first is the notion that when a new discovery is made, it shows the previous conceptions on the subject to be untrue; from which unscientific people draw the very natural conclusion that if today's scientific ideas show those of yesterday to be wrong, we need not trouble about them, because they may themselves be shown to be wrong tomorrow. It is difficult to imagine any statement more opposed to the facts. The progress of knowledge does indeed correct certain details in our ideas, but the main result of every great discovery is to establish the existing doctrines on a firmer foundation, and give them a deeper meaning.
>
> 'The second false notion is that the progress of research involves increasing specialization: and specialization, we are told, means knowing more and more about less and less. This last phrase has undoubtedly brought comfort to many people who know practically nothing about absolutely everything. But it is entirely untrue that as knowledge grows, its conclusions are based on a narrower range of observations. Nothing is more remarkable in the immense development of physics and chemistry during this century than the way in which it has brought to bear on the same problems the results of the investigation of widely different properties, obtained by widely different experimental methods.'

All these outside engagements, on top of his normal duties of teaching and lecturing, delayed the '4 books and new editions'. But what delayed them most was the forward march of the subject. Even the *Covalent link in chemistry*, which appeared in 1932, and was based on Sidgwick's Cornell lectures, contained much that he had not known when he was there. New methods of physical measurement had been developed to determine the distance between linked atoms, the force necessary to move them nearer or further apart, the energy of formation of the link, and the inter-valency angles. Wave mechanics which in 1927, 'had not yet reached the stage at which one can say definitely how it is to be translated into physics' had begun to throw light on problems of chemistry. The study of electrical dipole moments was already recognized as of great importance for the elucidation of structure, though it was then, for practical purposes, barely four years old. Lewis had originally assumed that the covalent link gave rise to non-polar molecules: in his view polar and non-polar were synonymous with electrovalent and covalent compounds. But that this was too narrow an interpretation of the facts had been shown by Debye so long ago as 1912. It was in 1928, however, during a conference in Munich in which Sidgwick took a leading part, that the real significance of dipole moments began to emerge, and since then the application of theory had been rapid. Over one-third of the new book was devoted to a description of the different methods of measurement of dipole moments, and to a critical survey of the existing—and growing—literature of the subject.

Thus, what with one thing and another, Sidgwick was able at this time to clothe his thoughts with greater precision, and, in particular, to bring a mass of evidence in favour of his contention that 'there are two possible forms of linkage between the atoms in a molecule, and that the two can in practice be sharply distinguished' . . . and also 'that links in which both kinds play any considerable part—links which are truly intermediate between an electrovalence and a covalence—rarely if ever occur'. The last chapter was devoted to a discussion of stereochemistry in the light of the new physical evidence which had accumulated in the last few years. The book was widely read, and certainly helped to give a feeling of reality to the chemist's sensory images; but it suffered, so it seems to me, from the inclusion of too much detail for its purpose, and from the attempt to reach conclusions with a precision that was hardly warranted by the state of knowledge at the time. It is not, perhaps, unfair to say that it was the least successful and most ephemeral of Sidgwick's works.

By 1932 Sidgwick had abandoned his intention to revise the *Organic chemistry of nitrogen* after writing four new chapters; so when the *Covalent link in chemistry* was out of the way he was free to concentrate on volume 2 of the *Electronic theory of valency*. He made a start on this early in 1935, and in the summer was writing to America: ' I have accumulated a lot of material, but I don't want to spend all my time in accumulating and not to live to use it.' He soon found that he had taken on a far greater task than he had anticipated. The science of inorganic chemistry had been given a new lease of life through the discoveries and theories of physicists, and it became almost impossible to keep abreast of the flood of papers on new classes of compounds and new elements. And then there was wave mechanics to be reckoned with. Quantum mechanics had been bad enough for a non-mathematician to swallow and digest; but wave mechanics was worse still. It could not be neglected, however. Had it not produced the concept of resonance, which, as he wrote in his introduction to the new edition of the *Organic chemistry of nitrogen* is the most important development which structural chemistry has had since it was extended to three dimensions by van't Hoff in 1874? Fortunately he had able expositors close at hand, and he refused to be baulked by mathematical difficulties. In his Presidential Address to the Chemical Society in 1937 he said:

> 'The general conclusions of the theory of resonance are of great practical importance, especially to the organic chemist. But it is not to be expected that more than a very few organic chemists will have either the time or the ability to master these mathematical operations. It is therefore obviously our duty to express—or to induce the wave-mechanicians to express—the results in the simplest terms that are possible, and this can really be done quite easily. The use of the technical language of wave mechanics for this purpose is quite unnecessary and in fact misleading, for it makes the non-mathematical reader think that as he can't understand the language he can't under-

> stand the conclusions either, which is quite untrue; and not uncommonly, I think, it leads the non-mathematical writer to believe that if he knows the technical terms he must understand their meaning, which is by no means always the case. It was well said by Clerk Maxwell: "for the sake of persons of different types of mind scientific truth should be presented in different forms, and should be regarded as equally scientific whether it appears in the robust form and colouring of a physical illustration, or in the tenuity and paleness of a symbolical expression".'

It was not so much the mathematical as the literary language of the wave-mechanicians that he objected to. Resonance was a bad name; for it implies that a molecule is oscillating between two states, which is what the theory denies. It reminded him, he said, of a remark by Mrs Wilfer, one of his favourite characters in the works of Dickens, of which he was a master:

> 'When I use the term attractions, I do so with the qualification that I do not mean it in any way whatever.'

However he did not encourage chemists to rechristen the phenomenon: 'for the present we have to accept the terminology of the physicists'.

I do not know when the full magnitude of his task broke upon him, and 'Volume 2' was abandoned in favour of an entirely new treatise. Not before 1939, I fancy, if indeed so early. By then he had revisited the United States six times. America drew him like a magnet. He loved the country; few Americans can have known it so well as he did. Before he died he had visited 46 of the 48 States, and had at least one friend in each. No British scientist was better known in the United States. He was made a Foreign Member of the American Academy of Arts and Sciences in 1938; but by then he had become, so to speak, an honorary American.

His visit in 1939 was the last stage of a journey round the world via Australia, where he attended the meeting of the A.N.Z.A.A.S. in Canberra, and lectured and took part in several discussions. He had a new experience there, for his old pupil David Rivett drove him from Melbourne to Canberra through Gippsland when it was ablaze with the worst bush fires within living memory, and the pall of smoke, cutting off the sunlight, compelled the car to slow down to a crawl with headlights on early in the afternoon. Sidgwick thoroughly enjoyed the weirdness of the scene, which he regarded as a realistic glimpse of Inferno. After the meeting he was driven to Sydney, and then crossed to New Zealand before returning across the Pacific to Vancouver.

The war of 1939 to 1945 was really a blessing in disguise for Sidgwick—or rather for the accomplishment of his purpose. Travelling was out of the question; even visits to London were rare, and held no attractions. Oxford, of all English cities, was the least affected by war, especially if one was a bachelor don living in College. More important still, from his point of view, the gradual cessation of purely scientific work meant that the flood of new information abated, became a stream, and then a trickle. He was able to get abreast of the literature at last. The chief worry now was whether his health, which was steadily deteriorating, would last out. It did: his will-power and his sense of humour sustained him.

The book, which had grown to two volumes of 750000 words, was finished at the end of October 1947 but for a final revision and the compilation of an index. The first part was sent to the Clarendon Press early in 1948. The Press was too optimistic about the probable date of publication, which called forth a typically Sidgwickian outburst later on:

> 'Feb. 22, 1950
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> Dear ——,
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> "Hope deferred maketh the heart sick." The Press promised me that my book would be out in early summer 1949—in late summer 1949—in Oct. 1949—in Nov. 1949—in January 1950—and finally and definitely (on Feb. 9) that it would be published on March 9. Now Mr —— writes to say that it will be published on March 23—after term. I don't suppose this statement is any more true than the previous ones. We now know what the promises of the Press are worth.
>
> When the reviewers say, as they certainly will, that the book is not up to date, I shall have to explain that this is largely due to the interminable delays of the Press.
>
> Yours regretfully,'

A soft answer, delicately pointing out the difficulties of printers so soon after the war, and the delay in preparing the index, subdued his wrath. He had only to possess his soul in patience for another month. The Press kept its last promise; and a week later, uplifted in spirit though enfeebled in body, Sidgwick sailed once more for the United States. He gave a few informal lectures, and ranged over the length and breadth of the country and into Canada, revisiting friends and places that he had not seen for eleven years. He went again in 1951, knowing full well that he was running the risk of shortening his life. But it was worth it. The warmth of his reception at the meeting of the American Chemical Society in New York, which he attended, will not be forgotten by those who were present. He was very feeble then, but still indomitable. He spent most of the three months of his absence in the company of Lee Davy, the dearest of his younger friends, who had been his research assistant at Cornell. He returned home to die.

> 'But he had revisited his old friends, he had received their congratulations on the completion of his great book, he had been as he had hoped to the giant atomic energy plant at Oak Ridge, and he had seen, as he had always wanted, the maple leaves of New England in their autumn splendour. His own autumn, too, had not lacked colour, and was fruitful to the last, the winter mercifully brief.'*

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* *Oxford Magazine*, 1 May 1952 (C.N.H.).

The chemical elements and their compounds is a monumental work of scholarship. As one reviewer (D.Ll.H.) wrote: 'never before has the content of chemistry been reviewed with such penetrating perception of its structure and such illuminating criticism of its philosophy.' Its aim was to discuss in detail the properties of all the elements and their compounds in the light of modern theory; and especially to bring the same order into the study of inorganic chemistry as had existed so long in organic chemistry. No one had attempted a task of such magnitude before; and it is safe to say that no one else could have achieved it with such remarkable success. It contains nearly 10000 references to original papers, the greater part of which had been studied and documented, and their essential points stored in Sidgwick's capacious mind, which never seemed to reach saturation. It was not to be expected that a work of such dimensions could be altogether free from errors and omissions; indeed the book was still hot from the press when chemists from many countries wrote to combine with their congratulations gentle complaints that their own work had been overlooked or misinterpreted. Other criticisms have been made—not without foundation. But few great works of art or scholarship have been free from imperfections; and I can imagine no more instructive task for a young chemist than to compile a list of the mistakes in Sidgwick's last and greatest book. It will take him many months of labour, and into many fields of knowledge, still only partially explored. For the mature chemist it is a mine of information into which he cannot dip at any point without being led to read on by interest in the subject matter and the attraction of the style. Sidgwick's own estimate of its circulation—'judging from the number of enquiries I get about the book's progress from both sides of the Atlantic'—was from 3000 to 5000 copies within three years. Up to date about 9000 copies have been sold. It will be surpassed in time; but it will long remain a standard work, and a pattern for future authors to emulate.

Sidgwick's place in the history of chemistry is assured. His own researches, embodied in a long list of contributions to scientific journals, are not outstanding for originality. He was a natural philosopher rather than a researcher; inductive rather than deductive; a weaver rather than a spinner. He would weave the threads of the discoveries of others into an ordered and harmonious pattern on a grand scale. He exerted a great influence on his contemporaries. His wide interests and knowledge, his forceful and clear speaking, illumined all discussions, whether in private gatherings or before large audiences. He was a most welcome visitor to international conferences, a cosmopolitan in the true tradition of science.

On Oxford he has left a mark which will be slow to fade. When he first came to Oxford as an undergraduate chemistry had only just begun to revive, largely through the work of College tutors in their own laboratories. The University Department of Chemistry was badly equipped, and lacking in leadership. Odling was, even then, rather an interesting link with the past than a guide to the future. Physicists had few contacts and no common ground with chemists. Long before he died Oxford was recognized as a leading school of chemistry throughout the world, and physicists and chemists were bound together by covalent bonds of interest. Sidgwick had a large share in this transformation through his personality and powers of leadership.

When he retired from the Delegacy of the Clarendon Press, after nearly thirty years service, his colleagues caused to be recorded a minute which includes the following paragraph:

> 'The deliberations of the Delegates and of the Finance Committee have gained by his power of analysis, the lucidity of his expression and, upon occasion, the pungency of his wit. The width of his knowledge and range of his friendships have been of great value to the Delegates in their dealings with books and authors.'

'The pungency of his wit.' What memories that phrase recalls! His wit was proverbial, and could most certainly be pungent. A story is told how, on an occasion in Congregation, when he was destroying with relentless logic the arguments of an opponent, one by one, an older member was overheard to mutter: 'It's like hammering nails into a coffin.' That is a perfect description of Sidgwick in one of his gladiatorial moods, which could be aroused by a conviction that an injustice was being done, or a wrong policy pursued. His strength in argument lay not only in his clarity of thought, and in the right choice of words which came to him without hesitation, but also in his aggressive commonsense. He would pounce like a tiger on any loose, inaccurate or pretentious statement. His friends did not escape. 'He's bitten most people' said one of his admirers, 'he's bitten me, I suppose he's bitten you?' Some were bitten so often that they became immune; others, more bold, would joyfully enter upon the unequal contest and derive great entertainment from it. He sometimes talked for victory, like Dr Johnson. He enjoyed himself on those occasions; and who would not with such weapons always at the ready? He made no attempt to conceal his personal prejudices, which were usually irrational and often productive of his wittiest remarks. But at heart he was the kindliest of men; genuine, sincere and lovable.

His emotions were strong and deep. He was passionately fond of music, especially in his younger days, when it was not unusual for a pupil to call on him for a tutorial and find him deep in the score of some classical music in preparation for an evening concert. He could play no instrument but the obsolete pianola, on which, strange to say, he became expert enough to please others besides himself, and which remained in use until it was superseded by a borrowed broadcast receiver. In his reading he was omnivorous; an apt quotation was always on his lips in conversation; and his retentive memory, constantly enriched by his travels and experiences abroad, made him a most delightful companion. He was a real humanist, old style.

He seemed quite indifferent to his immediate surroundings and to his personal comforts. A stranger, entering his rooms for the first time towards the end of his life, and observing the shabbiness of the furniture and the squalor of his bedroom, might well have inferred that he was in the presence of one of the poorest members of the University, instead of one of the richest; an opinion which he would have had to revise had he stayed to enjoy Sidgwick's hospitality. As a host he was supreme, always courteous and attentive, as good a listener as he was a talker, which is an unusual combination. As a guest he was welcome wherever he went and could be relied on to make a success of any party. An American admiral who sat next to him at lunch said afterwards to his host: 'That was a remarkable man I was talking to: are there many like him in Oxford?' 'Oxford's full of them', was the unblushing reply.

His indifference to his surroundings often seemed to extend to the troubles and difficulties of others, but this was due to a certain lack of imagination on his part. He could never have realized how exacting he could be of the efforts of friends who helped him in his work or on his journeys; nor how his colleagues and the College staff strove to protect him from the privations of war. He was curiously parsimonious in small ways; it really pained him to to have to pay highly skilled indexers of his books at a rate which an unskilled labourer would scorn. But he was a most generous man in big ways, not only in his private benefactions, which were noble, but also in the credit he would give to others. His health was robust up to well past middle age; but physical infirmities began to grow on him after the age of 60. He met them stoically, seldom complained, and his sense of humour did not desert him on his deathbed.

In 1950 some of Sidgwick's old pupils commissioned John Merton to paint his portrait. When it was finished it was presented to the College. I went to see it in the autumn, hanging in the College Hall. It is a striking portrait of Sidgwick in his old age, but many of us will prefer to remember him as he was in the fullness of his vigour. The old man himself was beside me, gazing steadfastly and thoughtfully at his image. 'What was he thinking?' I wondered. Death could not be very far off now; and death, for him, meant oblivion. He had no belief in an individual after-life. Still, his work was done; and it was good work. His books would be treasured in the College library: his name would live for ever in the College records. It was a pity that he had no sons to carry on the magnificent Sidgwick tradition of scholarship and learning: but perhaps one day his American godson Nevil would come to Oxford, and find a special welcome for his sake in Lincoln College.

H. T. Tizard

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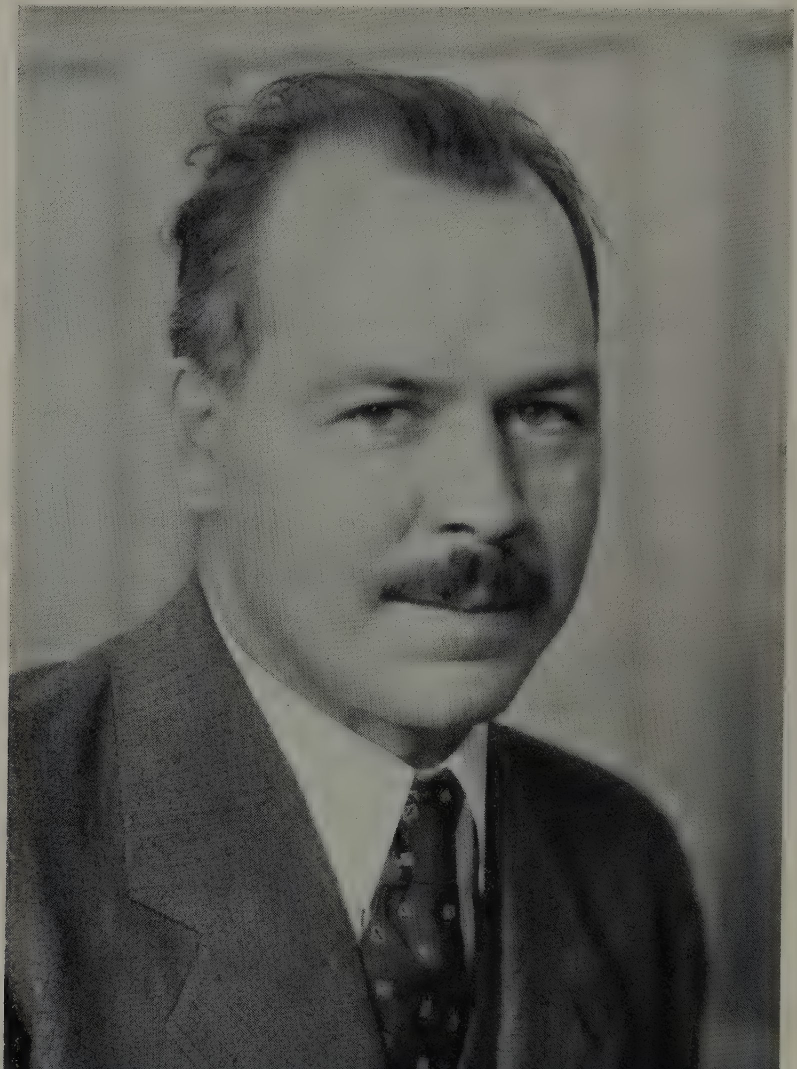

NICOLAI IVANOVITCH VAVILOV

1885-1942

N. I. Vavilov was born in 1885. The son of a textile manufacturer, he came from an able family. His brother, now deceased, was a distinguished physicist and a past president of the U.S.S.R. Academy of Sciences. In 1913 and 1914 Vavilov worked with Bateson at the newly established John Innes Horticultural Institution. He returned to Moscow in August 1914, on the S.S. *Runo*, which struck a mine on the voyage home and caused the loss of his valuable experimental materials. During the First World War he began his well-known explorations for cultivated plants. In 1916 he visited Persia and the surrounding countries, principally in search of materials for his cereal collections, though he had already done much experimental work on the systematic relationships of cereals. In 1917 he went as professor of agriculture, botany, and genetics to Saratov. Later, as head of the All-Union Institute of Plant Industry and as the successor of Philipchenko in control of the Bureau of Genetics of the Academy of Sciences, his work came to be recognized by Lenin, who gave him the joint post of President of the Academy of Agricultural Sciences and Director of the Institute of Applied Botany. Thus in 1921 at the comparatively early age of 36, Vavilov was enabled to carry out his vast projects for agricultural research. In the course of a few years he set up more than four hundred research institutes and experimental stations, covering the whole area of the Soviet Union from Leningrad to Vladivostock. Several of these had as many as two hundred research workers and it is estimated that by 1934 their staffs comprised over 20000 workers. Notwithstanding these vast administrative duties, Vavilov still found time to indicate to his colleagues the precise scientific details of the research programmes entrusted to them. He kept in constant touch with all the important work of the research stations and he controlled policy less by active direction than by guiding and sustaining research wherever he found it to be good.

Being convinced that the cure for plant diseases was to be found in the relatively new science of genetical engineering he set to work to explore the genetical variability of cultivated plants in what he thought to be their centres of origin, either primary or secondary. To quote Vavilov's own words: 'The Institute of Plant Industry has organized some seventy expeditions to various parts of the world. The objects of these expeditions were briefly, to collect practically every variety of economic plant which might be of use to the U.S.S.R., and to study the diversity of varieties of cultivated plants by employing the methods of the taxonomist, cytologist and biochemist. In understanding this work we have been prompted by the actual needs of plant breeding, which involves some of the most pressing practical problems in our large scale agriculture. It was evident to us that in order to conduct any rational large-scale work in plant breeding it was necessary to master exhaustively the whole initial varietal potentialities of the world as well as to learn about the nearest wild relatives of our useful plants', and to illustrate the vast scale on which he worked it may be mentioned that the collection of wheat grown and maintained near Leningrad amounted to 30000 varieties. Numerous studies of the taxonomy and variability of this wealth of material were published in his journal, the *Bulletin of applied botany, genetics and plant breeding*, which became an international organ of great repute.

Animal variability was not neglected and numerous centres were established for the study of the origin and distribution of the main domestic animals.

Expeditions revealed the great wealth of economic plant material of Afghanistan, Abyssinia and China as well as Central and South America. The expedition to Abyssinia, Vavilov carried out single-handed. He was given the sum of £1 000 by Lenin for his total expenses. For six weeks he did not even take off his overcoat. During the day he travelled and collected. When night came he flung himself on to the floor of some native hut. He ate whatever native foods were available. Dysentery afflicted him throughout his expedition but he returned with several thousand specimens some of which proved to be of great value to the plant breeders.

The collections were also to be the raw materials of new theories on the origin of cultivated plants. His basic idea was derived from plant systematics. It was that the geographical centre from which a cultivated plant spread was marked by the greatest genetic diversity and also by the greatest concentration of dominant genes. The meaning and validity of this contention have been disputed on both special and general grounds. His theories nevertheless have made possible the combined and rigorous systematic and cytogenetical treatment of variation within species of cultivated plants. In theory, Vavilov marked the first great advance on De Candolle. In practice he laid the foundations of all future improvement of crop plants. His discovery that the ordinary potato, *Solanum tuberosum* is only one of a large number of species with edible tubers, domesticated in South and Central America in prehistorical times, led to the establishment of the Empire Potato Collection maintained at Cambridge, which, fragmentary as it undoubtedly is, lies at the basis of potato breeding in Britain and elsewhere.

Vavilov had a mind that never slept and a body which for its capacity for enduring physical hardships can seldom have been matched. He could work twenty hours a day for weeks on end. But in spite of the Napoleonic traits and a certain flamboyant showmanship his intellectual integrity was never in doubt. Whenever he met another man of science for the first time he would ask, 'What is your philosophy?' meaning with what biological first principles, if any, do you work? His own attitude is well illustrated by a quotation from his last publication, *The new systematics*, (Oxford 1940), 'We are now,' he writes, 'entering an epoch of differential ecological, physiological and genetic classification. It is an immense work. The ocean of knowledge is practically untouched by biologists. It requires the joint labours of many different specialists—physiologists, cytologists, geneticists, systematists and biochemists. It requires the international spirit, the co-operative work of investigators throughout the whole world. . . We do not doubt that the new systematics will bring us to a new and better understanding of evolution, to a great increase in the possibilities of governing the processes of evolution, and to great improvement in our cultivated plants and domestic breeds of animals. It will bring us logically to the next step; integration and synthesis.'

Vavilov had just under twenty years to develop his ideas and to get his Research Institutes into going order. His views on genetics and evolution were not popular in government circles, where a naïve belief in the inheritance of acquired characters was given wide credence. His influence waned before the virulent attacks of Lysenko who became the tool of the Lamarckian elements in official circles. This conflict came to a head in 1939, Lysenko emerging as the victor. Soon after, Vavilov lost his official posts and Lysenko became the new President of the Academy of Agricultural Sciences. But though Vavilov was latterly thought little of in his own country, his fame abroad steadily increased. He was asked to be president of the International Congress of Genetics in 1939, an honour which, after first accepting he had to renounce when the Russians decided to take no part in the Congress. In 1942 he was elected a foreign member of the Royal Society, though he probably did not know this.

Vavilov spent all his life collecting and observing and arranging facts and ideas. He spoke several European and some Asian languages fluently, and with the same enthusiasm with which he studied their agriculture he interested himself in the customs, arts and music of the peoples he sojourned among. Physically he was rather short and dark, of a stocky build and a somewhat Tartar cast of countenance.

According to Professor H. J. Muller, he was arrested in 1940 during the period of the Hitler–Stalin pact, as an alleged British spy and for following Huxley. He was sent to the far north-east of Siberia, probably in 1941, and died there in 1942 apparently largely as the result of the hardships he had suffered since 1940.

So died one of the greatest men that the Soviet Union has produced. A world-wide host of friends have lamented his death. They are not likely to forget his Robesonian depth of voice, his Falstaffian breadth of gesture. And science at large will remember his achievement, an achievement that survives his own meteoric career and tragic end.

For the compilation of this obituary, I have made extensive use of the joint obituary published in *Nature*, in November, 1945 (Vol. 156) by myself and Professor C. D. Darlington, F.R.S. I am indeed indebted to Professor Darlington for permission to make use of that part of the obituary for which he was responsible.

S. C. Harland

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